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ORIGINAL ESSAYS.

*An Experimental Inquiry into the Botanical History, Chemical Properties, and Medicinal Virtues of the Spiræa Tomentosa of Linnæus.* By ELIJAH MEAD.—\*

BOTANICAL HISTORY.

THE Genus *Spiræa* has long attracted the attention of botanists, for the number and beauty of its species. The Genus is composed of woody and herbaceous plants, consisting of many ornamental flowering shrubs, and floriferous herbaceous perennials, all of hardy temperament. The several species are adorned with spear shaped, pennated, and tri-foliate, decompound leaves terminated by spikes or clusters of pentapetalous flowers.—(*Mawe's Univer. Gard. and Bot.*)

They are for the most part habitants of the colder regions of the north of *Europe, Asia and America*. NUTTALL remarks, that this genus is almost equally divided between this country and Siberia.

The term *Spiræa* seems to have been borrowed from THEOPHRASTUS, whose *Σπειραία* is supposed to be one of the species of this pretty genus. Most botanists however re-

\* This paper is the substance of an Inaugural Dissertation, submitted to the public examination of the Faculty of Physic, under the authority of the Regents of the University of the State of New York, for the Degree of Doctor of Medicine, at the late Medical Commencement.

fer it to the *Spiræon* of PLINY, (from *Σπειρα*, a rope) these shrubs being slender and flexible. LINNÆUS described thirteen species ; fourteen have since been added to the genus, making twenty-seven, thirteen of which, as far as I have been able to learn, are natives of America.

MÖENCH proposed, some time since, to form several of these species into a new *genus*, under the name of *Gillenia*, which has recently been adopted by BARTON and NUTTALL, who include two North American species, viz. *trifoliata* and *stipulacea*. The propriety of this arrangement however is questionable.

The species *tomentosa*, which I have chosen as the subject for the following remarks is a hardy perennial plant, peculiar to this country. The beauty of its foliage and elegance of its flowers, attracted the notice of that distinguished Florist, P. COLLINSON, *Esq.* who in 1736, introduced it into England, where it is now cultivated in gardens and pleasure grounds as an ornamental shrub.\*

The *Spiræa* belongs to Class XII. *Icosandria*, and Order IV. *Pentagynia*, of the sexual system of LINNÆUS ; and to the *Natural Order Rosaceæ* of MON. JESSIEU. The following general character is given by LINNÆUS. (Gen. p. 862.)

Cal. *Perianthum* monophyllum, semiquinquefidum, basi planum ; *laciniis* acutis, persistens.

Cor. *Petala* quinque calyci inserta oblonga,--rotundata.

Stam. *Filamenta* viginti plura, filiformia, corolla breviora, calyci inserta. *Antheræ* subrotundæ.

Pis. *Germina* quinque seu plura ; *styli* totidem filiformes, longitudine staminum. *Stigmata* capitata.

Per. *Capsulæ* Oblongæ, acuminatæ, compressæ, bivalves.

Sem. *Pauca* acuminata, parva.

The *Spiræa tomentosa* is found in abundance throughout most parts of the United States. PURSH speaks of this species as growing in Canada ; upon the Alleghany mountains ; and from New-York to Carolina. The following is

\*Likewise in the garden of the King of France as marked in the Catalogue by Desfontaines P. 205.



his amendment of WILLDENOW's specific description.—*Folius lanceolatis, inæqualiter serratis, subtus tomentosis; racemo terminali, composito, confertifloro floribus 5-gynis.* This plant is generally known through the country by some one of the following names, viz. *Steeple-bush, Hardhack, Downy Spiræa, or Scarlet Spiræa.* I have seen it in great abundance in the low grounds of Massachusetts. It arises in many thickly set branches from the same root, and grows to the height of from three to four feet. It flowers in July and August, and gives a scarlet hue often, to widely extended pastures. Dr. BIGELOW speaks of it, as very common in the environs of Boston, and known for having the dried tops of the last year's fructification conspicuous among its purple flowers; the leaves nearly oval, thick and tough, dark green above, whitish downy underneath; flowers small blue, inclining to purple, in long conical branches on the ends of the stems.

Professor IVES, of Yale College, informs me that he has often gathered it for medicinal use in the low grounds in the vicinity of New-Haven. Dr. TORREY, of New-York, tells me he has seen it at Weehawk, and found it more abundantly in the low lands of New-Jersey.

The *S. tomentosa* is often found growing with the *S. salicifolia*, to which it bears considerable resemblance. The following distinction is given: the *tomentosa* differs from the *salicifolia*, in having the leaves more deeply and unequally serrate, tomentose beneath; the terminating compound racemes closer and longer; the flowers of a deeper red, and eight times smaller; the fruit villose. The stalks are slender, and branch out near the ground; they have purple bark covered with a gray mealy down; leaves smaller than those of the *salicifolia*, downy and veined on the under side, but of a dark green above. Branches terminated by thick raceme of flowers, branched towards the bottom into small spikes.—(*Mart. Mill.*)

#### CHEMICAL PROPERTIES.

Before I speak of the medicinal virtues of the *Spiræa*

tomentosa, I shall make a brief inquiry into its chemical properties. In the following analysis, the experiments are designedly restricted to those only which illustrated the most important proximate principles of the plant, the best means by which its virtues may be procured, and the most eligible forms in which it may be exhibited as a medicine.

#### EXPERIMENT I.

Four drachms of the roots, stems and leaves of the plant, properly prepared, were severally boiled in eight ounces of distilled water; after boiling to half the quantity, the respective decoctions were strained, and the evaporation completed in a sand bath. The roots yielded *seven grains* of extract, which possessed considerable tenacity; it was of a dark purple colour inclining to red; its taste was pleasantly astringent and slightly aromatic. The extract from the stems weighed *eight grains*, and only differed from the last in being a little more astringent and less aromatic. From the decoction prepared from the leaves, *forty-eight grains* of a very pure and elegant extract were obtained. When warm, this extract was so cohesive, that it could be drawn out to the length of several feet, or spread into thin laminæ, which transmitted a beautiful deep red light. In this state it was easily moulded into pills: but when cold, it was nearly as brittle as *Kino*, and more dense than this substance, exposing when fractured brilliant even surfaces. It was actively astringent, and considerably, though pleasantly, bitter to the taste.

#### EXPERIMENT II.

Equal quantities of the bruised roots, chipped stems and dried leaves, were separately digested six days in alcohol. The alcohol upon the roots and stalks imbibed their colouring matter; that upon the former being slightly discoloured, while that upon the latter was changed to a light green, resembling in colour the woody fibres in the

recent state ; otherwise neither, indicated any of the sensible properties of the plant.

The alcohol in which the leaves were digested, soon assumed a dark green colour, and a slightly astringent taste. By evaporation it yielded an inconsiderable quantity of extract, less pure than that obtained in the last experiment. It was of a dark green colour, and of an unpleasant styptic taste. These alcoholic tinctures were not rendered turbid, or changed by the admixture of water.

### EXPERIMENT III.

A strong filtrated decoction of the leaves, prepared with distilled water, was submitted to the following re-agents.

*a* When a solution of Animal Gelatin was gradually added to a portion of the fluid, there ensued a copious ash coloured precipitate ; which after standing a few hours, became dense, leaving the supernatant fluid transparent, and almost colourless.

*b* With the Muriate of Tin, there was a dense yellow, precipitate.

*c* When a solution of the Acetate of Lead was added there was a copious gelatinous precipitate.

*d* The Nitrate of Silver gave a curdy brown precipitate.

*e* The Sulphate of Alumine caused a flocculent precipitate.

*f* A solution of the Salts of Iron changed the fluid to a dark colour ; and after standing some time, there was a copious dark green precipitate.

*g* With the Nitrate of Lime, a light brown precipitate.

*h* A delicate fawn-coloured precipitate was exhibited by the Sulphuric and Muriatic Acids.

*i* No immediate change ensued by the addition of the Carbonate, or the Siliciate of Potass.

*k* Alcohol and Ether induced no change.

*l* No change by the Vegetable blues.

## EXPERIMENT IV.

Three drachms of the leaves of the *Spiræa* were infused in six ounces of distilled water; this infusion was clear, resembling in colour Madeira wine, and exhibited to the taste the peculiar bitter and astringent properties of the plant. It was subjected to the same series of re-agents, as was the decoction in the last experiment, and with the same results, except that the several changes induced were less conspicuous. Three drachms of the infused leaves yielded by evaporation *eighteen* grains of extract, which was more bitter and less astringent than that obtained by boiling.

## EXPERIMENT V.

One drachm of the flowers and fruit of the *Spiræa*, was infused in three ounces of boiling water.—This infusion was pleasant to the taste, resembling very much a strong infusion of Bohea Tea, but its sensible properties were not so peculiar as to indicate the presence of any medicinal virtue, which does not exist in greater abundance in the other parts of the plant: it was not thought necessary, therefore, to subject it to a series of chemical tests.

## EXPERIMENT VI.

Three drachms of the leaves were boiled in six ounces of pure water, until all the virtue was extracted, which is yielded to that menstruum. The same leaves were then infused in proof spirits, a little below the boiling temperature, for two hours: when filtrated the fluid showed the presence of a small portion of colouring matter, but it indicated no other sensible property of the plant.

## EXPERIMENT VII.

The aqueous decoction of the last experiment was treated with a solution of Animal Gelatin, until there ceased



to be a precipitate ; after percolation, the fluid was nearly colourless, and imparted but little astringency to the taste. Half of this filtrated decoction was evaporated. It yielded a dark-coloured extract, which only resembled that obtained in the first experiment, by being bitter to the taste.

The remaining portion of the fluid, was subjected to the following tests :

*a* The Muriate of Tin, which caused a yellowish precipitate.

*b* The Sulphate of Alumine gave a brown precipitate.

*c* There was an immediate change to a dark colour by the Sulphate and the Muriate of Iron ; and after standing some time, there was a copious dark precipitate.

*d* With a solution of the Acetate of Lead, no immediate change was induced ; but the fluid soon became thickly interspersed with delicate yellow flakes.

*e* When the Nitrate of Silver was added, there ensued a flocculent fawn-coloured precipitate ; and after standing, the supernatant liquor became darker than the precipitate.

*Inferences deduced from the foregoing Experiments.*

I. From this experiment, we learn that every part of the plant possesses medicinal properties, in a greater or less degree, and that they are all readily yielded to water.

The roots afford a very pleasant extract, but in a smaller proportion than either the stems or leaves ; the quantity varying probably according to the season of the year in which they are taken from the ground.

This extract seems to possess some advantage over that from the other parts of the plant, inasmuch as it is a little aromatic, and therefore better fitted for cases of great irritability of the stomach, a symptom sometimes of no small importance in the diseases of children in which astringents are indicated.

The extract from the stalks has no superiority to that from the leaves, and as the quantity obtained from them is comparatively small, this preparation is considered as unworthy of particular remark.

In the leaves were found in great abundance the most active properties of the plant; they yielded to a watery decoction, a mild and pleasant bitter, and an active astringent, and if the decoction be evaporated, these principles are obtained in a solid extract, equal in weight to *one-fifth* of the leaves in a dried state.

II. This experiment shows that the *Spiræa* yields its virtues but sparingly to Alcohol. The admixture of water with the several alcoholic tinctures, inducing no change, proves the absence of any Resinous principle

III. The copious insoluble precipitate which took place in this experiment, on the addition of Animal Gelatin, (*a*) demonstrates the presence of a large proportion of *Tannin*.

From the brown precipitate with the Nitrate of Silver, (*d*) and the dark colour given by the Salts of Iron, (*f*) we infer the presence of the Gallic Acid, which appears in a combined state, as shown by the test *l*.

A considerable portion of the extractive principle resides in the leaves, as indicated by the tests *b* and *e*.

This plant appears to afford no Gum or Mucilage, otherwise, they would have been pointed out by the tests *i* and *k*.

IV. It appears from this experiment, that all the proximate principles of the *Spiræa tomentosa* are obtained by simple watery infusion, as proved by the several re-agents; but though the comparative quantities of Tannin in the infusion and decoction were not particularly noticed, the probability is, that it is proportionably less in the former than in the latter, for the extract from the infusion was obviously more bitter and less astringent to the taste, than that procured from the decoction.

V. This experiment shows that the flowers and fruit are only valuable in common with other parts of the plant.—

VI. All the virtues of the plant are yielded to water, and therefore this is its proper menstruum.

VII. In this experiment we find, that after all the Tannin is separated by animal gelatin, there remained a considerable proportion of bitter extractive matter combined with gallic acid. The former of these is indicated by the tests *a* and *b*, and the latter clearly proved by *c d* and *e*. It appears also from this experiment, that the astringent property of the plant resides principally in the Tannin.

### MEDICINAL VIRTUES.

The use of the *Spiræa* as a medicine, as far as I have been able to extend my inquiries, is but little known, and I believe no mention has been made of it as such by any writer. It is surprising that a plant peculiar to our own country, so abundant, and truly medicinal, should have remained thus long unnoticed by most American practitioners. The particular history connected with its first introduction into practice, I have not been able to learn; its use has been, I believe, principally confined to a small district in Connecticut; and for some years it has been prescribed as a valuable medicine by the most respectable physicians of New-Haven, Hartford, and Middletown.—Professor Ives has long considered it as one of our most valuable domestic remedies, and in many cases prefers it to others, belonging to the same class. The high opinion which he entertains of this article, is expressed in the following quotation from his letter in answer to one in which I requested a detail of cases; “Singular as it may appear, I have no cases, nor could my medical friends who are in the habit of using this article, (the *Spiræa tomentosa*,) furnish me with any. It would be as difficult for me to give cases of the use of the *Spiræa* as of *Catechu* or *Kino*:—The general indication of the article at this time only employs our attention.”

On the subject of the early use of this plant, Dr. TULLY of Middletown, remarks, "I have been in the habit of using the *Spiræa tomentosa* more or less ever since I commenced the practice of Physic, which was in the year 1811. I first learned its use from Drs. COGSWELL and STRONG, of Hartford, who adopted it from its popular reputation."

Every part of the *S. tomentosa* is medicinal, but the leaves, as has been shown, are most worthy of notice. The plant should be gathered about the third week in August, during the vigour of its fructification. The leaves may be preserved in a dried state, and used in decoction or infusion, or an extract can be easily made by boiling any quantity of them in water, and straining and evaporating the liquor. The evaporation may be conducted by boiling over a slow fire, or in a sand bath, till the liquor becomes a little inspissated; after which it is better to complete it in the sun.

This extract undergoes no change from keeping, and is generally the most eligible form for exhibition. The medium doses of these preparations are from four to six grains of the extract, repeated as the nature of the case may demand; from one ounce to an ounce and an half of the strong decoction, and of the infusion, which makes a pleasant tea, the patient may drink *ad libitum*.

I have said that the *S. tomentosa* belongs to the class of astringents, and from its sensible properties and its effects upon the system, as well as from the foregoing analysis, it has unquestionably high claims to that rank as a medicine. Observation has not enabled me to define precisely the peculiar virtues of this article, so that I shall not attempt to point out *every* disease in which it may be advantageously used; but from the very respectable testimony which will be introduced in addition to my own knowledge of its effects, I have much confidence in commending it *generally as an active astringent, a mild tonic, and, in moderate doses, a medicine that will seldom or never disagree with the stomach.*—



It will be perceived, therefore, that it may be very properly administered in some one of the following diseases, when they are unconnected with symptoms of fever; and that it is particularly indicated in such *forms* of them, as are accompanied with local or general debility.—The *Spiræa* has been prescribed with much benefit in the secondary stages of diarrhœa, and dysentery, in cholera infantum, and other bowel complaints of children. The efficacy of this remedy in some forms of these diseases, has been increased by combining with it small portions of Ipecacuanha. In diarrhœa originating in warm climates, the *Spiræa* has been given with obvious and complete success. I can speak of its efficacy in the case of a gentleman, who arrived in our city during the last summer, after spending some time in a southern climate. He was afflicted with a troublesome diarrhœa, notwithstanding the use of remedies ordinarily prescribed in such complaints. I directed him the extract in doses of six grains; under the operation of this medicine, he was suddenly and permanently restored to health.

“I have used,” says Professor Ives, “with obvious benefit, the extract of *Spiræa* in cases of diarrhœa originating in hot climates, combined with opium, in doses of from two to four grains. I have likewise used the entire plant in decoction, in cases of diarrhœa and dysentery. I have more generally used the extract, it being a more convenient form. To young children with cholera infantum, and other bowel complaints, in which astringents are indicated, I prefer the extract of *Spiræa* to *other* astringents, giving it in solution in water. In slight cases of dysentery without fever, this article has been used with success.”

On the medicinal powers of the *Spiræa*, I have the following from Dr. TULLY.

“The cases in which I have found the greatest benefit from *Spiræa*, are the chronic diarrhœa of seamen,

which is contracted in tropical climates, or is the sequel of some acute disease; the secondary stages of diarrhœa and dysentery of our own climate; the secondary stages of cholera infantum in children that have arrived at such an age, as to be able to take a bulky medicine; and likewise in apthæ and ulcuscula-oris. I have likewise used it with occasional success in several diseases of the urinary organs, such as Gravel and Hæmaturia. In bowel complaints, as a general rule, I prefer the extract to the decoction. In these cases I have frequently given to an adult two common sized pills every two hours, which I consider a suitable quantity. The decoction I make as strong as can be conveniently taken, and give from an ounce to two ounces, with the same frequency; but circumstances vary the doses. As a general rule, it produces no operative effects that prevent free use, or require great nicety of weight or measure in its preparation or administration. But after all I have more commonly used it as an adjuvant to other means, than as a principal remedy. I am decidedly of the opinion that its efficacy is greater in comparison with other articles of the same class, than the intensity of its astringency would lead us to suppose. I have remarked this both in bowel complaints and in thrush, &c.

I have formerly been in the habit of using extract of *Spiræa* to give consistency to pills of Acetate of Lead and Opium, for Hæmorrhages, under the idea that it increased the efficacy of the compound."

As a local application, Dr. IVES of this city has used the extract in solution in cases of gleet, fluor albus, fungous ulcers, &c. with decided benefit. In the former of these diseases, I had a very fair opportunity to test its virtue as an astringent in the case of a young man, who contracted gonorrhœa for the first time. Previous to his calling upon me, he had used the more common and popular remedies for this disorder, without permanent benefit. There

was considerable inflammation at the time I first saw him, accompanied with chordee; after these symptoms were in a measure relieved, there still remained a very-troublesome gleet. I then prescribed the aqueous solution of extract of *Spiræa* by injection; this was used four or five times in twenty-four hours, gradually increasing its strength. After the first day, the discharge was evidently diminished; in four days he was relieved from every unpleasant symptom; and in ten days he left the city, since which I have not heard from him.

I gave some of the extract to a medical friend, who used it successfully in two similar cases. In cases of obstinate and debilitating discharges in puerperal women; and in cases of retained placenta, giving rise to alarming typhoid symptoms; attended with colliquative diarrhœa; and when the stomach has become too irritable to retain other medicines, the *Spiræa* has had the most happy effect, in composing the stomach, in restraining preternatural evacuations, and in giving healthy vigour to the lax state of the bowels. In these cases its operation is certainly more than astringent.—The two following cases are interesting illustrations of its efficacy. For the first I am indebted to Dr. Ives of New-Haven, and to Dr. Wood of this city for the second.

“I was called in consultation in a case of a female about two months after parturition. The disease was not attended with much fever, but loss of tone of the stomach and bowels. The evacuations were frequent. The common astringents, (astringents were evidently indicated,) such as Catechu, Kino, and other vegetable astringents, were used, but all of them excited vomiting after a few doses had been given. I recommended the extract of *Spiræa* in doses of four grains, to be repeated four times in the day. The diarrhœa was speedily checked, and the patient fast recovering. The attending physician omitted the *Spiræa*, and commenced the use of other astringents with no better

success than at first. The extract of *Spiræa* was given again, and others omitted. Under the use of this article the patient was soon restored to health. From the botanical affinities of the *S. tomentosa*, from its operation in this and other cases, I was led to the conclusion, that in addition to its astringent properties, it possesses the power of a diaphoretic."

"I used," says Dr. WOOD, "the extract of *Spiræa* in a case of miscarriage, which took place about the third month of pregnancy. The *foetus* was expelled without much pain, and the secundines were retained from four to five weeks after. Frequent hemorrhages took place during this time, which together with the putrefaction of the placenta, so much exhausted the patient as to bring on a diarrhœa, with other symptoms of hectic. In order to restrain the discharge from the bowels, different astringents were made use of.—The patient's stomach having become irritable, so as at times to reject almost every thing. The exhibition of the *Spiræa* was suggested by Dr. G. SMITH, the physician in consultation. The extract appeared evidently to check the discharges from the bowels, and no unpleasant effects were produced by the medicine. The distressing effects which opium produced on this patient, precluded very much the use of it. This circumstance rendered it more particularly desirable to select some remedy, which might produce the desired effect without deranging the stomach; this was happily accomplished by the *Spiræa* in the form of pills."

This remedy will be found particularly valuable in cases of diarrhœa supervening Typhus fever, and may be prescribed in such graduated doses, as shall prevent the discharge from exhausting the patient too much, without the danger of improperly checking critical evacuations. It has likewise removed this very unpleasant and debilitating symptom in a case of Phthisis Pulmonalis, and thereby evidently prolonged the lives of patients labouring under this disease. I am indebted to the politeness of Dr.



STEARNS, one of the Trustees of this College, for the following case, which is much in point upon this subject.

“The extract of *S. tomentosa*, with which you supplied me, I used in a case of Diarrhœa from Phthisis Pulmonalis; although I was not prepared to expect any permanent benefit from any medicine in that case, I was however perfectly satisfied with its effects.” It was a case of extreme debility in the vascular system, unaccompanied by any symptoms of inflammation or febrile action, with an universal torpor of the Liver and a suppressed state of its secretions. By the previous use of calomel and other medicines, the Liver had been excited and the secretion of bile restored in such profusion, as to stimulate the intestines into preternatural action. An obstinate diarrhœa ensued, which ordinary remedies proved insufficient to restrain. I then submitted the *Spiræa*. Two pills of the extract were given every four hours.—In four days the frequency of the discharges was considerably diminished, and in a week they became regular and natural. The violent tendency to diarrhœa from this time was evidently subdued, for though it not unfrequently recurred, it was always immediately restrained. Although this did not prevent the fatal termination of the Phthisis, the primary disease, there is much reason to believe that it contributed much to retard that event, and conduced to the comfort of the patient, by removing a very troublesome and debilitating symptom.”

I know not that the *Spiræa* has been much prescribed in uterine hæmorrhage, but from its analogy to other efficient remedies in that complaint, and also from the testimony in the following extract of a letter favoured me by Dr. EATON of Brookfield, Massachusetts, I am led to believe that it will prove a valuable astringent in such troublesome and often dangerous cases.

“I have used” says the Doctor, “the extract of *Spiræa* with which you favoured me, in several cases, and am

pleased with its operation. I have prescribed it one case of uterine hæmorrhage successfully, after *other* remedies had failed."

In further testimony of the medicinal virtues of this article, I subjoin the following communication from Dr. A. W. Ives, of New-York.

"I began to use the *Spiræa tomentosa* about two years ago, since which I have had frequent occasion to prescribe it as an astringent. From its sensible properties, I was led to believe that the *Spiræa* possessed more virtues as a tonic, than most vegetable astringents; and this opinion was soon strengthened by the exhibition of it to my own child, in the early stage of Cholera, when there existed considerable febrile action. The symptoms of fever were increased, and I imputed it to the operation of the *Spiræa*. Since that I have prescribed it only in those forms of disease in which tonics as well as astringents were indicated. Under such circumstances, I have found it an efficient and valuable medicine,—indeed I know not that it has failed under my direction in any instance of producing a salutary effect. In a few instances, it appeared to be efficacious, when the ordinary means had failed. These were cases of chronic diarrhœa with general and local debility. A lady who for years had been subject to frequent attacks of that disease, was directed five grains of the extract of *Spiræa* three times a day. She had previously taken, with but little or no benefit, acids, alkalies, and astringents in various forms, besides resorting frequently, for temporary relief, to laudanum and other astringent injections. I was surprised to learn that the disease was suspended by a few doses of the extract; and the lady informed me but a few days since, that although it occasionally returned during the last winter (which she spent in Georgia,) it was subject to an immediate and complete controul by the *Spiræa*.

I have used this remedy uncombined with other medicines sufficiently to convince me of its astringent and tonic

powers, though in a few cases I have united it with ipecacuanha. As in diarrhœa, a dryness of the surface is generally an important feature of the disease, I am of opinion that the foregoing combination, after suitable evacuations, will be found one of the most eligible prescription in that complaint.

I have never prescribed any preparations of the *Spiræa* internally ; but the solid extract, I have given from one to five grains of it at a dose, according to the age of the patient. As a lotion, I have often used the extract in solution, and I think it one of the most convenient and efficient which can be prepared from the vegetable kingdom. As an application to ulcers with weak granulations of luxuriant growth, and “ as an injection in Fluor Albus and gleet depending on great relaxation, I have found it highly useful.”

The medicines already in general use possessing properties the most analogous to that of the *Spiræa*, are Kino and Catechu ; but the *Spiræa* must be regarded in many respects, equal, if not superior, to either of them. It can be procured at less expense ; an equal quantity of the extract possesses more virtue as an astringent ; all its properties are soluble in water. It is equally pleasant to the taste ; it never disagrees with the stomach, and what is of still greater importance, it may always be obtained free from adulteration. Whereas of the Catechu, it was justly remarked by Dr. CULLEN, “ We *never* get it pure, and this should lead us to endeavour to find for it a substitute of our own growth.”

#### EXPLANATION OF THE PLATE.

- FIG. 1. The Plant reduced to one-fourth its natural size.
- FIG. 2. A single flower of the natural size.
- FIG. 3. The same magnified about 16 times.
- FIG. 4. Germen and Styles magnified to the same size.
- FIG. 5. The five united capsules, Do. Do.

After the foregoing had been sent to the press, I was favoured with an interesting communication from Dr. MASON F. COGSWELL of Hartford, Connecticut. It contains facts so important in connexion with the subject of this Essay, and more particularly the circumstances in relation to its first introduction, I shall take the liberty of subjoining it at length.

"I most cheerfully comply with your request respecting a medicine, which I consider a valuable addition to the *Materia Medica*. About 12 years since, I was called into a neighbouring town to consult with one of my brethren, in a case of a lad 10 or 12 years of age, who was sinking under an obstinate diarrhœa, the consequence of a malignant dysentery. After examining the patient, we walked into the lots, the father accompanying us; while conversing on the subject, he plucked a branch of the *Spiræa*, which was then in blossom, and inquired if we knew what it was? He of course received a negative answer, as neither of us had noticed it before; on tasting it, its sensible qualities were such, as to induce me to propose the exhibition of it to our patient; my proposition was readily assented to, and he recovered without the aid of any other medicine. Pleased with its effects in this case, I determined to give it a fair trial in other cases, as soon as opportunities offered; accordingly, I gathered a considerable quantity, and very soon had an opportunity of testing its efficacy. In the succeeding fall we had more of cholera infantum than usual. Dr. STRONG and myself used it freely, and on the whole, were better satisfied with its effect than with those of any other remedy we had ever used. Dr. STRONG made an extract from the leaves, which, by the way, they yield abundantly, and to which he is partial, having used it much more than I have. I have used it sufficiently, however, to be satisfied with its efficacy. I gave a small quantity of it to Professor IVES, who was much pleased with its use; of this, however, he has probably informed you himself. It was some time before I found what the plant was; and if Dr. TULLY first learned its virtues from me, I was indebted to him for its Botanical history.

The account which I received from Mr. Butler, the parent of the patient, in whose case I first used it, was, that he had traced it to a *Mohegan Indian*; a source from whence most of our *new medicines* are traditionally derived: you will, of course, attach to it as much credit as it *deserves*. We have generally given it to children in the form of a decoction, by boiling it in milk and water, and sweetening it; in this way it forms a fine balsamic astringent, which is very efficacious. In chronic diarrhœa, especially in adults, the extract is singularly beneficial; in short, I consider the plant in the various forms in which it may be administered, as a very important addition to our class of indigenous astringent medicines."



*A Memoir on Accidents of the Ankle Joint, by John Baxter, M. D. Honorary Member of the Philadelphia Medical Society, &c. &c.*

MONS. DUPUYTREN, Surgeon in chief of the Hôtel Dieu at Paris, has very ably elucidated the principles, and improved the treatment of these accidents. The results of his observations and practice, are detailed in the *Annuaire Médico-Chirurgical*; a collection of papers by the surgeons and physicians of the Hospitals of Paris on the most interesting subjects that have come under observation at their respective hospitals. The work is published annually, under the direction of the Council General of the hospitals, and from the extensive field for observation on surgical and medical subjects, it promises to become a valuable source of professional information. The work is in 4to. with 14 plates, in folio. M. Dupuytren's paper is on fractures of the fibula and its complications. In my late attendance on this gentleman's practice, having been satisfied with the results, I wish to make them known to the profession.

"No part of surgery, says Pott, is thought to be so easy to understand, as that which relates to fractures and luxations." The validity of this belief, we see frequently controverted, even at this day, in the number of halt and lame meeting our eyes, from the reliance of people on their own knowledge, or on that of various impostors, pretending to derive their supernatural powers in this respect, from birthright or immediate divine gift.

Perhaps no accidents require more knowledge of the part, than those that happen to the ankle joint, either from its mechanism, the extent of parts liable to be injured, or the serious consequences which so frequently result from a wrong treatment; and notwithstanding the light which science and observation have thrown upon the subject, it

has hitherto been very deficient, and we cannot but allow with Mons. D——, that “there is not perhaps any surgical disease, the treatment of which presents more uncertainty, more deficiencies, and, in general, more inefficacy, than the fracture of the fibula accompanied with luxation of the foot; and there is none, if I mistake not, which, from the frequency and seriousness of the symptoms attending it, require more imperiously than this, a determined treatment, founded on principles established by experience.”

Notwithstanding the frequent occurrence of this kind of accidents, and the evil consequences (sometimes fatal) of neglect and faulty treatment, we are surprised at the little notice that is taken of them in surgical works, and the few writers that have treated particularly of them.

Among the ancients, we find the subject hardly mentioned. Hippocrates has nothing precise or satisfactory, and considers luxation of the ankle joint, when accompanied with an opening in the articulation, of so serious a nature, as to advise us to abstain from its reduction. After him, we have nothing of consequence, until J.L. Petit on Diseases of the Bones, (1723,) besides some cases in his posthumous works, which show the uncertainty of the treatment of that time. “We have nothing from this great surgeon in detail, and find him recommending pullies and machines, for what may be effected, if properly conducted, by the hand alone.”

David, in a Memoir read to the Royal Academy of Surgery, (1771,) attempts to restore the theory of counterstroke, as productive of fractures of the fibula; which is controverted by the observation and experiments of Mons. D——. The theory did not lead to any useful treatment, though it is still adhered to by some. The only case given by him, submitted to amputation two months afterwards.

William Bromfield, in his *Chirurgical Observations and Cases*, (1773, London,) enforces attention to the subject, and

establishes better than any previous writer, the indications to be fulfilled. The method of treatment he recommends, is somewhat similar to that of Mons. Boyer, but though the means are rational, the object is not obtained.

Mr. Pott, (1775,) in his remarks on Fractures and Luxations, has spoken of this accident with his usual perspicuity. He has enforced the necessity of the integrity of the fibula, to the integrity of the joint. Vol. 1. p. 328. Had not Mr. Pott been too much led away by his favourite theory of demiflexion of the limb, on which he put his whole reliance, he would not have overlooked the consequence to which his doctrine leads. Mr. Hey has said, demiflexion alone is not competent to the treatment and the prevention of the consequence of this accident.

Pouteau, in a memoir, published in 1783, says that these fractures may take place from the action of the muscles, alone in taking a false step; and he indicates the part of the fibula fractured, but admits the insufficiency of surgery, to prevent deformities, and abandons them to nature and exercise.

In the remarks by Fabre on this subject, (1783,) a prosecution is related to have been commenced against the surgeons, on account of the deformities and lameness produced by their treatment.

Dessault gave little attention to this subject. He has published but two cases of this kind, (1801,) from which he concludes, that it is not so serious an accident as is generally supposed; but, says Mons. D——, this is controverted by daily experience.

M. Richerand, in a Treatise on the Bones, (1803,) enriched by some observations by M. Chaussier on the curvature of the fibula, recommends the use of Scultet's bandage, with "splints placed at the side of the leg, to prevent the deviation of the foot outwards."

Mr. Castella published an Essay, (1808,) in which is related

his own case, treated by splints, as is usual in fractures of the leg, leaving a deformity and lameness eighteen months after the accident.

Charles Bell, in treating of fractures of the fibula, distinguishes those produced by indirect and direct causes. He considers the former the most serious, and accounts for them in the same way as David. His treatment consists in placing a splint on the outside to push the foot inward, with lint between it and the limb, and maintains the whole by an 18 tailed bandage. The leg is flexed on the thigh, and laid on the outside, which must increase the pressure of the splint on the leg by its weight; besides, that the weight of the foot tends to turn it outwards, and counteract the indication:

Mr. John Howship has more recently published (1816) remarks on this accident, by which it would appear that lateral splints are still in use in England. Two cases related by him show the incompetency of such means, and that a machine has become necessary to remedy the inconveniences resulting from the accident. This machine he describes.

From this sketch of the state of the subject, mostly drawn from the French surgeon, it will be seen that this branch of our art is still very imperfect, and affords ample room for improvement. I know of no treatment in our own country which avoids the evil pointed out; and it remains now to see what Mons. Dupuytren has done to overcome the difficulties attendant on cases of this kind.

### ANATOMY OF THE PARTS CONCERNED.

Before treating of the accident, it may be proper to take a short view of the parts concerned in this articulation, and call to mind our knowledge of its structure. Mons. D. speaks very much in detail on its anatomy, but does not seem to have contributed any thing new on this part of the subject.



This structure consists of hard and soft parts, of resisting and moving powers.

It will be recollected, that the tibia and fibula concur to form a mortise for the reception of the head of the astragalus, the two sides of this mortise forming the maleoli, the external of which, formed by the fibula, is considerably longer than the internal. These bones are connected to each other, and to those of the foot, by strong ligaments, especially by the one passing from the internal maleolus to the astragalus, calcis, and naviculare, at the inner ankle; and three at the outer ankle, of which, "the middle one, strong and thick, passes downwards from the end of the fibula, to be inserted into the outside of the os calcis. The anterior and posterior pass also from the external maleolus, and are inserted into the anterior and posterior portions of the astragalus." (Wistar.)

The moving powers, or muscles that move the foot, may be considered as of four different kinds, viz. as they produce flexion or extension of the foot, or its motions in depressing and elevating the toes, and the motions of adduction and abduction, or turning them inward and outward. The foot then is acted on by these different kinds of muscles, whose tendons run over the joint.

The extensors are necessarily the strongest, and are aided by the flexors of the toes as auxiliaries.

The flexors are less powerful, but more numerous, and have the extensors of the toes as auxiliaries.

The adductors and abductors by themselves, are perhaps nearly equal. The former, however, have the disadvantage of acting on the shortest lever, but the auxiliary aid derived from the extensors of the foot, give them the superiority in a state of integrity of the joint, owing to their being then less restrained by the internal maleolus, which is shorter; so that, when the resistance to each is the same by fracture of one or both maleoli, the abductors have the advantage, for the inequality of the levers is not suffi-

ciently compensated by the increase of auxiliary power, received by the adductors, only so long as the resistance is unequal; but the abductors being strengthened by the aid of the flexors of the foot, acting with them, both together turn the foot outward and upward when the maleoli are broken.

Thus it would seem, that the difference in length of the maleoli, and therefore of resistance, is what determines the point in favour of the adductors. The other soft parts, which surround this articulation, are those common to the other parts of the body, and must suffer more or less in every accident of the joint; the nerves, blood-vessels, lymphatics, cellular membrane, and common integuments, as well as the different sheaths of the tendons, being injured, produce various complicated symptoms, as pain, redness, effusion, swelling, tension, ecchymosis, inflammation, and suppuration, which are to be combated on the ordinary principles of surgery.

The articulation of the foot is thus surrounded by powers which tend both to its preservation and derangement; the ligaments and bones are so adjusted as to afford protection against derangement of the tendons by their muscles, which according as they act, contribute to both.

But other causes may so intervene, as to overcome the preservative powers, and produce an action in the other powers, to the destruction of the articulation in its functions. The violent transmission of the weight of the body in an inclined position, may do this, and thus produce derangement by indirect means. Direct causes may do this by violently disorganizing the parts, and amputation is then called for.

Violent transmission is the most usual cause, producing derangement in different degrees, according to the extent of the fall, or kind of accident, occasioning sprains, fracture of the fibula, or luxation of the foot in different directions.

Before treating of each, of which separately it may be

necessary to examine the principle which occasion the difference between each of them. On this point Mons. D. has much valuable matter; but in searching authors for information on the subject, very little can be found; it is either passed over in silence, or the *methodus acci-dendi* very incorrectly stated.

"In considering, says Mons. Dupuytren, "the column which transmits the weight of the body to the ground, as formed of one piece, we see that column diminishes as it approaches the ankle, where it widens to form the maleoli which embrace the astragalus; it follows necessarily, that that part above the maleoli, at which it is the weakest, should be more easily fractured than elsewhere. But this column is not formed of one piece, two bones concur for this purpose, which cannot be possessed of great mobility, without danger to the articulation in the transmission of the weight of the body, nor of complete immobility without inconvenience to the motions of the feet; thus these two pieces are so united, as to afford a mobility which may not be prejudicial to the solidity of the joint, and yet to permit some motion so "as to facilitate and extend its lateral movements:" besides the ligaments which unite these bones, are so strong, that if after dividing the interosseous ligament, we attempt to separate them violently from their attachments, we shall in some instances tear away the outer lamina of the tibia, but oftener, the whole of the fibula.

"The column thus constituted, has for its base the feet; a kind of arch, narrow behind, wider forward; and flexible and elastic, by means of the articulations and ligaments which unite its different parts."

The articulation by which the leg is joined to the foot, is not placed on the middle of this arch, but at one end, so that a perpendicular line continued from the centre of the former, falls on the superior and internal part of the astragalus," which gives to the abductors of the foot, the ad-

vantage of the longest lever to act upon: "considered in relation to the axis of the foot, the point of incidence of this line is such, that it divides the foot into two unequal parts, the anterior of which constitutes three quarters nearly, while the posterior forms one quarter more or less: these two parts are united on the astragalus, which thus becomes the moveable centre on which rests the weight of the body in the different circumstances in which the different motions of walking, running, leaping, &c. place it."

The skeleton cannot maintain an equilibrium of itself. For this end the muscular powers are added; but "the position of the body on the foot, rendered more difficult by the narrowness of the base, will bring into simultaneous action all the muscles of the limb, and as the slightest inclination of the body tends to destroy the equilibrium, we shall constantly find the flexors, the extensors, the abductors, and the adductors, necessarily redouble their contractions to maintain it.

The extension of the foot forward and backward, renders the equilibrium less difficult to be maintained in those directions; but sideways, it is much more difficult to prevent the body being thrown from the centre of gravity, the point of incidence of which is thus thrown more on the inside: thus it is that in progression, the weight of the body is necessarily thrown from one foot on the other, and the point of incidence is constantly changing from the centre of gravity, and the support of one foot is not given to the other, until the ligaments are stretched to their greatest extent.

"But the muscles which give to the articulation of the foot its force of situation while standing, are also the power which tends to preserve it from the derangements and disorders to which it is exposed by the movements they produce; and Mons. D. thinks, that from the distance of the abductors and adductors, this power is not so placed, as to counterbalance the inequality of the powers of the branches of the two levers, which is in favour of the abductors:



and receives additional power from the predominance of the abductors over the adductors, by means of the flexors, which are so disposed, as that "they are found placed on the outside of a line drawn from the heel to the joint of the foot, dividing the foot into two equal parts, and which act in consent with the abductors," and therefore turn the foot outward when the maleoli are broken.

"In an ordinary state, the transmission of the weight of the body in a perpendicular line, does not produce any derangements; the powers and resistances placed round the articulation, being sufficient to prevent them.

But if the force applied even in a perpendicular direction, be too great the consequences are often serious, but do not come within the scope of our present remarks.\*

Any cause which will destroy the equilibrium of the body, while changing the centre of gravity from one foot to the other, and which, consequently, will throw the line of transmission of the body's weight, in an inclined direction to one side or the other, may produce this accident. The causes which lead to this, are too numerous, and too obvious to every one, to need enumerating. The sudden loss of equilibrium carried to a certain extent, and then recovered, may produce a wrench or strain of the ligaments, without farther accident than the rupture of some small blood-vessels of the part; but when carried beyond the powers of the resisting and safe guards of the part, farther mischief, as fracture and luxation, is produced. These resistances are, the maleoli and ligaments on each side,

\* Mons. D—— however gives a case of the removal of the astragalus, which was extracted with difficulty, by being wedged in the joint; having been turned upside down, and the inferior and posterior projection, which forms the lower articulation, retained under the tibia; but by passing a string under its neck, and elevating it firmly, it was extracted, with the exception of a small fragment, which came away afterwards. The man was on a ladder behind a wagon, which started forward, and he, to extricate himself, threw himself backward, lighting first on his toes, and then on his heels. This man recovered, but limped three years after.

and these ligaments must be ruptured, or else the maleoli, to which they are attached, give way.

“David, in 1771, in a memoir crowned with the prize by the Royal Academy of Surgery, attempted to restore the theory of fracture of the fibula by a counterstroke. According to him, the fracture of its lower extremity is the result of a violent movement of the foot outward, by which the fibula is strongly pushed from below, upwards, by the astragalus, and restrained above by its articulation with the tibia, and the strong ligaments there; but being obliged, nevertheless, to yield to the pressure and the action of the peronæi muscles, become more powerful by deviation of the foot outwards, is broken between these two points, and especially at the weakest, just above the external maleolus. This theory, continues Mons. D. seducing as it is, inas-much as it gives a simple and plausible reason for the fracture of the fibula and luxation inward, has the fault of being contrary to observation and experiment, which every day prove, that there are fewer sprains which carry the external edge of the foot outward, than those in which it is carried inward and downward, which produces fracture of the fibula.”

This accident, fracture of the fibula, is usually accompanied with luxation of the foot, almost always inward; and it should be remarked, that the French Surgeon has adopted, and with much justice, inward and outward luxation of the foot, in having regard to the manner in which the astragalus is turned, when that luxation is inward, and vicê versâ.

‘The due and proper use and execution of the office of the joint, says Pott, depend almost entirely on the perpendicular bearing of the tibia on the astragalus, and on its firm connexion with the fibula. If either of these be perverted or prevented, or if it be separated by violence from the astragalus, the ankle will suffer a partial dislocation internally.’

“When the fracture of the fibula happens, he continues,

but how this takes place, he does not explain, the inferior fractured end of the fibula falls inward towards the tibia, that part of the bone which forms the outer ankle, is turned somewhat outward and upward, and the tibia having lost its proper support, and being of itself incapable of steadily maintaining its true perpendicular bearing, is forced off from the astragalus inwards, &c. &c.”\* This is little satisfactory, by no means accounts for the accident, and is not true, inasmuch as it supposes that fracture of the fibula always precedes the luxation.

The theory of Mons. Boyer, which differs but little from that supported by David, is disproved by experiments. The astragalus, he says, presses the inferior extremity of the fibula from within, outward, and in abduction, the os calcis presses the same upward and outward, with a force equal to the weight of the body, augmented by the force of the fall: the immediate effect of the effort, to which the fibula is submitted, passes, in one case, to the ligament of its inferior articulation with the tibia; in the other, to the superior tibial articulation, whose surfaces are brought together with force, and the bands of the inferior articulation resisting, and the bone not being able to be displaced, either outward or upward, its natural bend must increase with a facility in proportion as it is already most distinct; hence, the solution of continuity which takes place in that of the length of the bone which offers the least resistance.

To all this Mons. Dupuytren opposes his doctrine, from which he has deduced his new method of treating this accident with great success, and which the writer of this has frequently witnessed during his late attendance on that surgeon's practice at the Hôtel Dieu.

To consider only, says Mons. D—— the volume and relative densities of the parts, we should be led to believe that the malleoli would offer a greater resistance than the lateral ligaments. Experiment, however demonstrate the contrary.

\* Pott's Works, by Earle. Vol. 1, 382.

## EXPERIMENT I.

Having firmly fixed the foot of a dead subject in a vice, so as to leave the articulation and maleoli free, incline with a moderate force, the top of the limb to one side, and the other, alternately. The ligaments towards which the leg is bent, are relaxed; those of the opposite side are distended, yielding a little, without being sensibly torn. This is what takes place in sprains.

## EXPERIMENT II.

If the limb be carried with still greater force outward or inward, a crackling noise will be heard, followed by a greater degree of mobility, showing that a resistance has been overcome. The compact tissue or the periosteum in which the ligaments are inserted, will be found torn off, while all the ligaments themselves are entire.

## EXPERIMENT III.

But if a still greater force and rapidity be used in either of the ways indicated, a crackling noise will also be heard, as in the preceding experiment, and a still greater mobility produced. Dissection of the parts will discover. 1st. If the leg has been bent outward, (which is equivalent to a movement of the foot in that direction,) a fracture of the base of the internal maleolus without a solution of continuity of the fibula, if fracture be the effect the effort; and a fracture of the lower end of the fibula, if the effort has not been expended on the fractured maleolus. 2d. When the limb has been carried inward, (equivalent to moving the foot in that direction,) a fracture of the inferior extremity of the external maleolus, will be found at a greater or less distance from its summit; but however extensive and violent the movement may be, no solution of continuity of the internal maleolus will be found: a separation or displace-



ment of the fragments of the fibula, never takes place in the last experiment.

“ In these experiments the foot is immoveable. If instead of that, the leg be made so, and the hold be taken on each edge of the foot, and it be bent in an opposite direction to the movements of the leg, the solutions of continuity will be found higher than in the preceding experiments.”

The results then of these experiments, are, that an effort made to produce abduction or adduction of the foot, determines, according to its extent, a straining of the ligaments, their separation from their insertion in the maleoli, and further violence produces a fracture of the maleoli themselves, and “ in violent movements of the foot outward, (abduction,) *the tearing of the lateral internal ligaments, or the fracture of the corresponding maleolus, always precedes the fracture of the fibula*; and in the violent movements of the foot inward, the fibula is almost always fractured, while the maleolus and internal lateral ligaments remain unhurt. In the two last cases the maleolus is broken by traction at the summit, which becomes more powerful as the lateral ligaments, changing their direction by the deviation of the foot, approach each other from a perpendicular direction of the maleoli.

These experiments show too, that when the fibula is fractured by abduction, it is not done by the pressure of the astragalus against the external maleolus, pressing the end of the fibula upward and outward; as has been supposed by the authors mentioned, but is occasioned by an alteration of the line of incidence of the weight of the body; which, instead of being transmitted through the tibia to the foot, passes through the fibula, which being too weak to support it, gives way at its weakest part, thus producing fracture, often luxation.

In the other case, viz. adduction, he will not allow pressure of the astragalus acts the same way, but attributes it

entirely to "traction." Perhaps it may be doubted if pressure has not some influence in producing it; but at any rate a displacement of the fragments of the fibula ought to take place, which we have seen not to be the case, in the second case of the third experiment. The pressure too, would fall principally on the strong bands that unite the tibia and fibula, and they would more frequently be ruptured or torn from their insertion; whereas M. D—— gives but one in 207 cases of fracture.

But we learn, from his practical observations, that the fibula may be sometimes fractured primitively, and the foot luxated inward; the first, however, is produced by the powers which tend to turn the foot inward, which are superior to their antagonists. They acting, as we have said, on a longer lever, have the advantage: but still, as they have a greater obstacle to overcome, they cannot exercise their advantage, until the resistance by some accident is removed and made equal to both, for if the outer or both maleoli are broken off, the most powerful, when brought into action, will turn the foot as that action tends, and that is outward: hence sprains and fractures of the fibula by traction, are most common, but when the adducting force has accomplished the fracture of the fibula, the abducting power brought into action by the struggles and continued efforts of the patient, produces a luxation of the astragalus inward, which luxation is by far the most common.

### LUXATIONS OUTWARD.

From what has been said, it is evident that luxations outward, are not so frequent as those inward. They are even rare, and were it not for the reasons stated, M. D. thinks it would be very difficult to account for the circumstance, since in sleep the club foot, and in most false steps and sprains the foot is turned inward, and the astragalus outward.

“The fracture of the lower end of the tibia, instead, as might be supposed, of turning the foot inward, is accompanied with luxation inward. It is only under certain circumstances of this fracture that the luxation outward takes place, and that is, where fracture of the fibula has occurred at the same time, and that of the tibia is oblique from without inward and upward, to which M. D. adds the resistance of the inferior fragment of the fibula. In this case, the foot is pushed up with the fragment of the tibia, the external malleolus presses against the external side of the foot, and enables the adductors to displace it outward.

“If the action of the muscles be added to the combined movements of the foot and leg, (see experiments,) and the mechanical power which produced them, which represents the weight of the body, we shall have an exact idea of what takes place during life in the production of sprains of this joint, and fractures of the fibula by indirect means.”

#### LUXATION FORWARD AND BACKWARD.

Of the former, I know of no case recorded by any author. Mons. Boyer considers violent extension of the foot necessary to produce it; the foot being fixed, and the person falling backward. The following experiment disproves this.

“By engaging the foot of a subject in a vice, and producing a violent movement of extension or flexion, a noise will be heard, as of the tearing of some of the parts, and dissection will discover the rupture of some of the ligaments from the bone to which they are attached. The same results are obtained by moving the foot instead of the leg. Thus it is, that sprains are produced, and it would seem, says Mons. Dupuytren, that luxation of the foot forward and those backward, ought to be produced in the same way; but this is not the case on the dead body, and in the living, they require a different mechanism : such, as having the foot fix-

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ed under a cross-bar, the leg being arrested by some neighbouring body, as a point of support in some part of its length, and transformed into a lever of the first kind, in which the power represented by the weight of the body, is at the superior extremity of the tibia, the fulcrum in the middle, and the resistance at the lower extremity in the ligaments on the opposite side of the joint, to which the weight of the body is transmitted."

But that the transmission of the weight of the body in an inclined direction, from behind forward, can produce the luxation backward, we have proved in the cases related by Boyer and Dorsey. The one related by the former happened by lighting on an inclined plane, with the foot extended on the leg, and the weight of the body falling on the foot. That of the latter occurred in descending a flight of stairs, and catching the heel of the shoe in the step, so as to fall backward. In both, the tibia sliding from behind forward, the line of transmission being oblique in that direction, the luxation was produced by the aid of the *gastrocnemii* muscles, which act when the fibula is fractured, (which is not however, always case,) but when it does take place, it is by adduction, is primitive, and the ligaments are usually torn. These ligaments, which M. D. thinks are to limit the flexion and extension of the joint, and the tendons running over the joint in the grooves formed in the bones and tied down by ligaments, forming a sort of moveable ligaments to the joint, are the principal resistances to be overcome. Luxation backward by violent flexion is opposed, by the neck of the astragalus, which meets the anterior edge of the tibia, before the centre of the astragalus has passed the posterior edge.

#### OF SPRAINS.

This subject has by no means received that attention which the frequency of its occurrence, the seriousness



of the accident, and the bad consequence of wrong treatment, or what is more common, no treatment at all, demand. How often do we see a sprain neglected, the patient limping about his accustomed employments, until no longer able to bear the pain occasioned by the inflammation, which the continual exercise of the joint, and the want of proper care, produces; and the person at last resorts to medical treatment, with a state of things which will require several bleedings, with confinement, and which may not after all, be sufficient to prevent suppuration, or perhaps ankylosis. But such a state of things, sometimes also, arises from the carelessness or ignorance of the practitioner, who looks on the accident as unworthy his attention, and thus lameness for life is entailed on the patient from the want of timely aid and proper treatment.

Scrofulous disease, in the form of white swelling, is not unfrequently called into action by this accident.

The manner in which sprains are produced, has already been alluded to, and it was proved, by the experiments on the dead body, that violent extension or flexion, adduction or abduction of the foot, will produce them in the ligaments on the side from which the limb is bent, and consequently that they may be produced on different sides of the joints, viz. before and behind, and at each malleolus. The two first, as well as the luxations in those directions, are less frequent than to either side, owing to the greater range the joint may take in flexion and extension, than in adduction and abduction. The ligaments on one side only, are usually injured: those on the side from which the foot is turned, as will be perceived on examining a patient with the accident.

Sprains by adduction, are more common than those by abduction, from the inferiority of the latter power in a sound state of the joint; and also as M. D—— says, because of the support given by the other leg, in sprains by abduction, which moderates the effort; but more probably from the

chance of the unequal transmission of weight of the body, in the various circumstances in progression, &c. in which this accident is produced, the centre of the base being usually between the feet, by which means the line of transmission is from within, outward, so that if the external edge of the foot be arrested by an opposing object, the weight of the body, instead of fixing the foot firmly to the ground, presses the astragalus outward; the internal edge is turned upward, and the sole inward; but the velocity being exhausted, and the assistance of the other leg being received, the equilibrium is restored. External sprains are more common than internal ones or luxations, though not more so than fracture of the fibula by traction. On which side soever the sprain is, it will be found that the foot has been turned to the opposite side, and when double, to each side, alternately. Fractures of the fibula often occur with sprains of the ankle joint. During 1815 and 16, the number of sprains of this joint, without fracture of the fibula, treated in the Hotel Dieu, were to those with fracture, as  $7\frac{1}{2}$  to 1.

M. D—— bears testimony to the good effects of rest and pressure, in the treatment of sprains, especially of the latter in those of the wrist, by making the hand immovable, by the application of long splints.

The cases admitted in the Hotel Dieu in 1815, were of the following proportions. Of 52 cases in females, the simple, to double sprains, were as 22 to 1,—external to internal,  $4\frac{1}{2}$  to 1,—simple and double, to complicated,  $7\frac{2}{3}$  to 1,—and of the right foot to the left, 3 to 1.

In 1816, of 45 cases of women also, the simple to double, were as 12 to 1,—external to internal, 5 to 1,—simple and double, to complicated,  $5\frac{1}{2}$  to 1,—of the right foot to the left,  $2\frac{1}{2}$  to 1.

In 1816, of 59 cases of men, the simple to double, were 16 to 1,—external, to internal, 12 to 1,—simple and double, to complicated,  $7\frac{1}{2}$  to 1,—and of the right foot to the left, 3 to 1.

The right is more frequently sprained than the left,

from the greater tendency to put it forward. Our author, like others, has treated this subject rather cursorily, and dismissed it with very few remarks on its treatment, but he has gone rather more minutely than authors in general have done, into the nature of the accident, though perhaps its relation and analogy to the other accidents of the ankle, leaves us little to look for on this point. Other parts beside the ligaments usually suffer more or less, as the capsule of the joint, the tissue of small vessels, and the tendons of the muscles, by which much pain, swelling, and frequently spasms of the muscles of the leg are produced.

In order to give due and proper effect to immobility and pressure, it is necessary to obtain them by proper means, and M. D——'s apparatus is admirably calculated for these purposes, and moreover does not prevent the application of cold lotions so beneficial in this accident. Most of the symptoms of sprain are common to fracture and luxation, as pain, swelling, and redness, but the distinguishing marks of these latter injuries are wanting.

#### SYMPTOMS OF FRACTURE OF THE FIBULA.

These symptoms are divided into two kinds, viz. presumptive and characteristic. The former are the kind of accident suffered; the crackling noise heard at the time; the seat of the pain; the difficulty or impossibility of walking; and the swelling. None of these can furnish a correct diagnosis, but can only excite suspicion of the accident.

The characteristic signs are twelve; the first four are those which appertain to fractures, generally; They are, the inequalities of surface over the bone; the unnatural mobility of the fragments; the crepitation sensible to the feel and the ear; and the displacement of the fragments. To those may be added, as appertaining to, and always attendant on this fracture, the mobility of the foot sideways.

“ These arise exclusively from the solution of continuity of the bone, and when the cause which has produced it does not cause other injury, they distinguish it from luxation, consecutive to the fracture.” The experienced surgeon will readily recognise them.

The remaining six, are rather signs indicative of the kind of luxation that accompanies it. They are, the change in the point of incidence of the leg on the foot ; the displacement of the foot ; rotation of the foot on its axis ; the extension of the space between the two malleoli ; the different angles formed by the tibia with the foot, and the remaining one, is common to both luxation and fracture, it is “ the sudden disappearance of all the symptoms on reduction, and the action of the muscles of the limb. This is more particularly remarkable in luxations than in simple fracture. Of the simple fracture, M. D—— makes two varieties ; first, when it takes place more than three inches above the summit of the malleolus ; second, when it takes place below. The first is always produced by a direct cause, and “ is never, and cannot be attended with a displacement of the foot,” except “ that a continuation of the same cause after having produced the fracture, may also determine either immediately, or consecutively, a luxation.”

The second variety is less than three inches from the summit of the external malleolus, most usually at two and a half inches, where the fibula takes a curve inwards, according to M. D——, from the pressure of the body of the muscle, and where it is smallest ; and is produced by an indirect cause and motion of the foot outward. It may occur, however, lower down, where the tibia is articulated with the fibula, by adduction but never below this point.

Fracture of the malleolus, which is most common, is produced by adduction, and may occur without displacement, when the effort has been exhausted in the fracture.

Simple fracture of the fibula not unusually attends sprains, and passes unnoticed if there be no displacement,



until some time afterwards, during the treatment. It is the next most serious accident the joint is liable to, resulting from violence carried beyond the point of producing sprain, and is always attended with more or less injury to the ligaments, and produced in adduction of the foot, except the cause be *direct*. In case it be caused by direct means, it will depend upon the place of fracture and the extent of injury, in what manner it is to be treated : for if it be above the point at which fracture takes place from adduction, it must be referred to fractures of the bones of the leg.

The number of cases of this kind, caused by traction simply, and unattended by farther mischief, is comparatively small, about one-tenth in two hundred and seven cases : for the struggles of the patient and the attempts frequently made to walk after this accident has taken place, occasions the abducting forces to turn the foot outward, and produce the luxation inward.

Where this fracture is united without coaptation of the fractured portions, more or less stiffness of the joint must remain : for in the displacement of the fragments, the malleolus externus is thrown outward, the foot and articulation deprived of its support, and a lameness and hobbling will be the consequence. To remedy this, the apparatus to be described is admirably well calculated.

M. D—— considers all other injuries to this joint, but as complications of the fractured fibula. Under his five first complications, may be considered the

### LUXATION INWARD.

This accident includes the rupture of the internal lateral ligaments, fracture of the internal malleolus at its summit, that of the lower end of the tibia, with which the displacement of the foot is a concomitant, and also an opening into the cavity of the joint with protrusion of the fractured end of the tibia.

This luxation may be preceded by fracture of the fibula, or precede it, as has been shown, in the former case, by *traction*, and in the latter, by the *weight of the body*.

The different degrees of injury at the internal ankle, do not materially effect the treatment, except the joint be opened, and the fractured tibia or the ligaments protrude through the opening, and even these accidents do not always call for amputation. These different injuries may usually be ascertained by examination in the first instance, but not always. The signs of this luxation are, the displacement of the foot outward, allowing a horizontal motion of the foot in that direction, on account of the restraining power being removed; the rotation of the foot on its own axis, which is a combined movement from within outward, and from below upward, so that the sole is turned outward, and the external edge of the foot upward; the increase of space between the two malleoli, which must be distinguished from the same appearance, when the foot is simply tumefied, and should be compared with the other foot; the obtuse and salient angle made by the tibia on the internal side of the ankle; the depression on the external side of the ankle, which is greater or less, according to the degree of luxation; the rotation of the foot outward, and fracture of the fibula; and finally, the sudden disappearance and reappearance, by the reduction and dislocation, which is no small evidence of the kind of fracture. Such are the distinguishing marks of this luxation, which, says Mr. Pott, 'in its most simple state, unaccompanied by any wound, is extremely troublesome to put to rights, still more so to keep in order; and unless managed with address and skill, is very frequently productive, both of lameness and deformity ever after.' Dessault tells us that there are great difficulties about this accident, but thinks amputation is too often resorted to. This also is the opinion of Boyer, who supposes it may take place without fracture, but he accounts for the accident very imperfectly.

The greater frequency of the luxation of the foot in this direction, has attracted the principal attention of surgeons, but its connexion with the fracture of the fibula has heretofore been neglected, or when noticed, not explained, or at least upon very wrong principles. The experiment on the dead body disproves conclusively the doctrine of counter-stroke, and goes to establish a more correct one in its place.

A plate is given us of the effects of inefficient treatment of *this luxation*, in which the deformity is admirably delineated, and shows us that some more successful treatment is necessary, than that usually pursued, and it is here that Mons. D——'s method appears to great advantage.

The LUXATION OUTWARD, occurs only with an oblique fracture of the tibia, from below upward and inward, which designates the kind of luxation. The obliquity not only influences the displacement of the fragments, but that of the foot also; and if to this be joined the resistance of the lower fragments of the fibula, pressing the foot from without, inward, producing a rotation of the foot on its own axis from within, outward, and causing a depression at the inner ankle, and a shortening of the limb, the luxation cannot be mistaken.

Luxation of the foot outward and upward, was once observed at the Hôtel Dieu; and constitutes the 8th complication of Mons. D. It occurred once only in fifteen years, in 200 cases of fracture near this joint. To produce it, it is necessary that the fracture should first take place, with a laceration of the tibial-peroneal ligaments, which in the experiment detailed gave greater resistance than the osseous texture. Besides the usual symptoms of fracture of the fibula, there existed a shortening of the limb; the width between the two malleoli was doubled, and from a consequent separation of them, the tibia was brought down to the sole of the foot, the astragalus ascended two inches, together with the external malleolus, and the whole of the foot was brought to the outside of the tibia; symptoms which

never accompany the fracture of the fibula alone, and left no doubt of the nature of the accident. The tension and tumefaction increasing, an attempt was made at reduction, but failed: the patient was then bled, and another attempt made, which succeeded, and the proper dressings were applied. On reapplying them afterwards, the luxation was reproduced by an unwary motion of the patient, but it was again reduced, the dressings applied, another bleeding practised, and in 36 days from the accident the patient was enabled to walk on crutches, as in common fracture, and in six months afterwards, no appearance of the accident could be discovered.

### LUXATION BACKWARD.

Besides that a partial luxation of the foot backward, is an attendant of other luxations of the foot, and complicated with displacements of the inferior fragments of the fractured tibia and fibula. It is also completely luxated backward, and the articulating surface of the tibia passed beyond and in front of the astragalus. The number of these accidents is comparatively small. Mons. D—— having met with but two or three in fifteen years, whilst he has seen several hundred inward.

The foot is drawn back by the gastrocnemii muscles, the tibia sliding forward on the astragalus. If the fibula only be fractured, its lower end is carried back with the foot, the fractured end points forward, the luxation is incomplete, and the foot turned outward; but if the tibia be also fractured, the length of the foot forward is less; the heel is longer, the displacement deranges the muscles and the tendons on the instep, and both malleoli remain behind.

### OF THE TREATMENT.

It was the advice of Hippocrates, to abandon these accidents to nature. J. L. Petit employed the stirrup with



the figure, of eight bandage: Lecat, pressure by compresses, so as to separate the bones of the leg; as did also Bromfield. Pott, demiflexion: and Dessault treated them as ordinary fractures of the leg, with compresses and lateral splints, the inner one shorter than the other. But it must be confessed that the method of Dessault is little better than the other methods, since the case of M. Castella and that of the soldier, recorded by Mons. D. both treated with side splints, recovered with lameness and deformity.

*Indications.*—When there is no displacement, rest alone is sufficient, as in fractures of the fibula higher up than three inches above the inferior extremity; but it may be as well to employ the dressings proper for consecutive derangement, although they are not directed by our author.

Immediate reduction of the dislocated joint in every instance, greatly expedites the recovery as by it, the pain and other distressing symptoms are in a great measure relieved. “I once advised, to wait until the inflammatory symptoms had abated, before attempting the reduction, under the apprehension that the violence necessary to be used, would only farther aggravate the pain and inflammation; but this is a mistaken notion, for by restoring the parts to their relative and natural position, we take the most effectual means of allaying all source of irritation. To delay the reduction and to temporize, says Mons. D. is to expose the patient to dreadful suffering, to spasms, convulsions, and tetanus, as well as to swelling, tension, compression, inflammation, extensive suppuration or gangrene, and frequently to death.”

*Of the Reduction.*—For this purpose it is necessary to overcome the powers which occasion the displacement, and these are the muscles. Pott first introduced this principle in the reduction of fractures, and it is equally applicable to the reduction of these dislocations: and for this purpose, those muscles which tend to displace the fragments should be relaxed by the position of the limb

and bones. Mons. D. often succeeded in reducing the luxation by diverting the patient's attention, by upbraiding him on some subject. In displacing the foot, the gastrocnemii muscles have very considerable influence. The flexion of the leg on the thigh, therefore, is always a matter of considerable importance, and will generally enable the reduction to take place by the hand, which otherwise would require a very considerable force.

But even after the luxation has been apparently reduced, and the joint restored to its natural state, the fracture will remain unreduced, and the fractured end of the inferior fragment will be depressed towards the tibia: to remedy this, has always been a desideratum. As the lower fragment has a sort of tilting motion (*en bascule*) at its articulation with the tibia, we may act on the lower end by traction through the medium of the external lateral ligaments, and for this purpose, the foot must be turned inward, as in adduction. This is Mons. Dupuytren's method.

The maintenance of the reduction proceeds on the same principle; it consists in turning the foot inward; by which means both the luxation and fracture are retained in their reduced state.

The apparatus necessary for this purpose is a cushion, composed by filling a linen bag about two-thirds full of chaff, which is to be about two feet nine inches long, four or five inches wide, and three or four inches thick, a splint nineteen totwenty one inches long, three wide, and three or four lines thick, and two rollers, five to six yards long each. These are all that is necessary.

The luxation having been reduced, if there be any; the bag of chaff having been doubled, is applied along the inside of the tibia, the thickest end just above the inner malleolus; over this is applied the splint, reaching several inches below the sole of the foot; these are secured to the leg, by passing one of the rollers round the upper end, just *below* the knee, the roller being carried round several

times, and secured. A space of several inches is left between the foot and the splint, and the second roller is now fixed on the lower end of the splint, below the lower end of the cushion at the inner ankle it is passed, round and drawn tight alternately over the instep and under the heel, including the splint, and forming a figure of eight. The apparatus acts as a lever of the first kind, the point d'appui of which is the cushion, where the pressure is greatest, and thus whilst the foot is drawn inward by the lower roller and the elasticity of the splint, the tibia is pushed outward by the bag of chaff, and traction necessarily produced on the lower end of the external malleolus, and the end of the lower fragment thus pressed outward. It is not sufficient to restore the foot to its natural position, but it should be 'turned as far in by the apparatus, as it was outward by the muscles.'—The only inconvenience which results from this, is the deviation of the foot inward, which may be remedied in a few days, by reversing the apparatus, and putting the splint on the outside of the leg.

To maintain reduced the luxation of the foot backward, the same apparatus is required, but differently applied. Here the luxation being reduced, the bag of chaff is applied behind the leg with the thickest end of the cushion pressing against the heel; the splint is then applied over it, secured as before, and the second roller passed round that and the lower end of the tibia, which is to be guarded from pressure by soft compresses. This roller is to be drawn tight, by which means the heel and foot is pushed forward, and the tibia at its lower end drawn back, and the intention completely fulfilled.

The greatest inconvenience likely to arise from this apparatus, is excoriation from pressure at the heel, and in front of the tibia. No mention is made of this inconvenience; but it may probably be obviated by the proper application of compresses.

Where the displacement of the foot is both backward

and inward, the apparatus must be applied to remedy that derangement which is the most serious.

Mons. D. recommends the apparatus applied in the first form in luxations outward, with deviation of the foot inward; but had he not asserted that he has employed them successfully in these accidents, we should feel inclined to doubt their beneficial application; for as it becomes necessary to apply the splint and compression on the outside of the leg, in order to bring back the foot from its deviation inward, the pressure is necessarily made more or less upon the end of the lower fragment of the fibula, by which it becomes depressed; unless indeed the point d'appui is placed lower down on the external malleolus, by which that may be pressed inward, and the fractured end of the fibula thus tilted outward.

The superiority of this apparatus to the former methods of treating these accidents, has frequently been tested and confirmed by comparison, in the cases at the Hôtel Dieu; and some cases are given to this effect:—Dissections after death, from other causes, have shown, that it fulfilled the intention, and that the ends of the fractured fibula were in complete contact. The reduction, and application of the apparatus, relieve at once the sufferings of the patient, the tension, swelling, &c. One case, however, is given, in which the apparatus increased the sufferings to such a degree, that the patient died, the resident assistant having neglected to remove the splint as ordered. On dissection, the sharp points of the fragments of bone were found piercing the muscles and integuments.

#### RESULTS OF THE TREATMENT.

The whole number of cases recovered of accidents to this joint, treated after this method in the city and in the hospitals of Paris, were 207, of which seven-twentieths were the right leg, and three-tenths of the left; six-tenths also,



were produced in the adduction of the foot; three-tenths in abduction; and the remaining one-tenth, by direct causes. Of the fractures, five-tenths were two inches above the summit of the external malleolus; three-tenths below two inches; and two-tenths above two inches.

Three-fourths were by indirect causes; one-fourth by direct; one-tenth were without displacement, and nine-tenths with, of which seven-tenths were displacements inward, and two-tenths inward and backward; of those inward, two-tenths were without rupture of the ligaments or malleoli, four-tenths with, and one-tenth with fracture of the internal malleolus.

Of the 207, but three were accompanied with simple luxation of the foot outward, and deviation of the foot inward. The fractures with laceration of the skin to those without, were as one to seventeen—seven patients had necrosis of the bones or tendons, and eight low delirium.

The ordinary cases required the application of the apparatus from twenty-five to thirty-five days. It was usually double that time after the removal of dressings, before the patient could be said to be perfectly recovered.

In almost all the cases, the foot was turned inward by use of the apparatus, but the action of the muscles and the splint placed upon the other side, sufficed to remedy this inconvenience. Only two in the 207 were a little deformed, and one only lost the use of the joint by achylosis; two hundred and two were cured, five died, three from the accident itself, and two from complications independent of it. Thus there was about one death to 41 recoveries.

Amputation was performed in none of these cases.

The concurrence of dreadful symptoms frequently attendant on these accidents, but common to others, and which require to be treated on surgical and pathological principles, we pass over.

Mons. D. arranges the complications under thirteen heads, viz. comminutive fracture; more or less disorder be-

neath the skin, and in the interior of the joint ; the rupture of blood-vessels, by which their contents are poured into the surrounding cellular membrane ; a solution of continuity of the integuments, either primarily or consecutively ; a swelling, tension, and constriction, with the consequent inflammation and suppuration occasioned by displacement of the foot, or spiculæ and fragments of bone, irritating the surrounding soft parts, and forming abscesses ; permanent pain resulting from laceration, giving rise to spasms and tetanus, of which last a case is detailed in all its horrors, the only one that came under his notice ; a low delirium unattended with pain, and not producing any lesion or mark of inflammation on the brain or its membranes, and which yielded to the repeated use of anodyne enemata ; necrosis of the bones and tendons ; and finally fevers of different types, according to circumstances.

The imperfect information to be gathered from the annals of surgery on the treatment of these accidents, and the inefficiency of the treatment heretofore employed, make us hope, that on further trial M. D.'s method will not disappoint us. The result of the experience here detailed is certainly satisfactory ; but how far he may be considered as the inventor of the plan he suggests, may perhaps admit of a doubt: for on referring to the *Chirurgical Works of Boyer*, whose name does not occur in Mons. D.'s paper, we find him, in vol.3. p.385-6, giving the following direction for the treatment of fractured fibula: " To maintain the reduction, we may take advantage of the structure of the limb, and by fixing the foot in a state of adduction, exert on the external malleolus, by means of the external lateral ligament of the joint, a sort of continual extension, by which this eminence is drawn downward, and outward, and the foot fixed solidly in its natural situation." This is evidently Mons. D.'s method, and his reasons for it are the same : but M. Boyer uses the bandage of strips, with an additional splint

1870

1. The first part of the book is devoted to a general history of the world, from the beginning of time to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

2. The second part of the book is devoted to a history of the United States, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

3. The third part of the book is devoted to a history of the various nations of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

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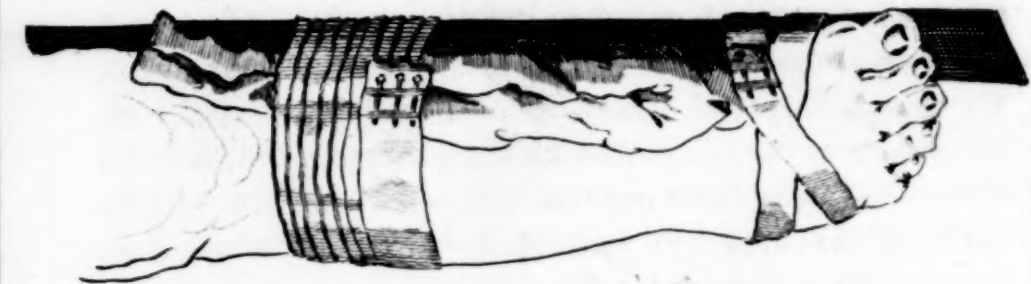
6. The sixth part of the book is devoted to a history of the various arts of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

7. The seventh part of the book is devoted to a history of the various professions of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

8. The eighth part of the book is devoted to a history of the various occupations of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

9. The ninth part of the book is devoted to a history of the various pleasures of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.

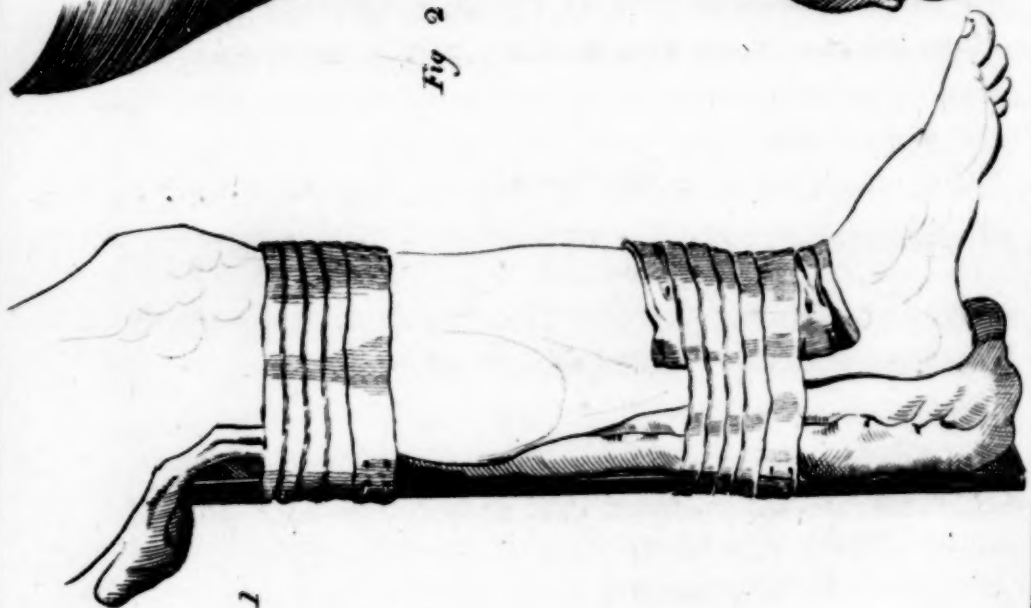
10. The tenth part of the book is devoted to a history of the various pains of the world, from the first settlement to the present day. It is written in a simple and plain style, and is intended for the use of schools and families.



*Fig 3*



*Fig 2*



*Fig 1*



outside with compresses, between it and the foot, to press it inward, and has not besides the bag of chaff on the inside.

He recommends the same treatment also in luxations of the foot inward with fracture. Vol. 4. p. 387.

Whoever may have the credit of first suggesting the plan, the experiments and inductions of Mons. Dupuytren, with but few exceptions, establish satisfactorily the principles on which these accidents may be accounted for, and clearly point out an improved method of treatment; founded on correct views, and deduced from just reasoning, which can alone establish a permanent practice either in Surgery or Medicine. All else will prove merely ephemeral.

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### EXPLANATION OF THE PLATE.

#### FIGURE I.

Represents the apparatus of Mons. Dupuytren for fracture of the fibula with luxation of the foot *backwards*.

#### FIGURE II.

Represents the appearance of fracture of the fibula with luxation of the foot inwards. The original of the engraving may be found in Pott's Surgical Works.

#### FIGURE III.

Represents the apparatus of Prof. Dupuytren for Figure II.

*Remarks on an Ulceration of the Mouth of a peculiar character, as it occurs in the New-York Alms House : By Dr. J. J. Howe, late Resident Physician to that Establishment.*

This disease, commonly called the *sore mouth*, or caries of the jaw, in the Alms House, has long been known in that institution; though I believe it is rarely met with in private practice. I have never heard of an instance of it out of the city of New-York. Many of those physicians who have had an opportunity of seeing it in the Alms House, consider it as a new disease, and so far as they are able to judge, heretofore unnoticed by any medical writer.

Some few years since, it was made the subject of an inaugural thesis, by one of the graduates of our university; but nothing concerning it, to my knowledge, has ever been laid before the public.

Having had the immediate charge of nearly all the children in the Alms House, for about three years and a half, as house physician to the Institution, I have had many opportunities to observe the disease, and to witness the efficacy of treatment, instituted for its cure: these circumstances, and the silence of others on the subject, I trust will be deemed a sufficient apology for the present paper.

The subjects of the complaint, according to my observation, are children from two to twelve years of age; I have never seen an instance of it in any person arrived at the age of puberty, or in a child previous to the appearance of the first set of teeth. When it has made its appearance in the Alms House, it is frequently observed to affect a number of children about the same time, and I have often seen in one room, where fifty or sixty children were lodged, from six to twelve, at once labouring under the same disease. The first intimation of its attack is usually a drivelling of their saliva: sometimes a swelling of the cheek, with a slight

excoriation or ulceration in the corner of the mouth, appear as early symptoms. On looking into the mouth at this time, an ulceration is observed along the edges of the gums, and frequently on the side of the tongue and inside of the cheek.

The ulcers on the gums are at first of a light ash colour, with sharp irregular and slightly elevated edges: as they extend, they soon assume a darker appearance, and bleed on the slightest touch. Those on the cheek and tongue, from being superficial and of a whitish appearance without hardness of the neighbouring parts, soon become excavated, of a brown colour, and surrounded by an extensive induration and swelling of the parts adjacent. The ulcers from the first are strongly foetid. The saliva is increased in quantity, and often acquires a brown or molasses colour, from the admixture of a bloody ichor discharged from the ulcers. As the ulceration extends, the teeth become loose, and drop out, or are easily picked out with the fingers. The alveoli are exposed, and become carious. The ulcer in the cheek advancing through its substance, perforates the integuments, and rapidly extends in all directions. It has been my good fortune never to have witnessed the disease in a more advanced state, than that above described; and in few instances when it has progressed so far. In one instance there was a perforation through the cheek, the size of half a dollar, and a considerable portion of the jaw bone exfoliated; but the disease was arrested, and the child recovered. In another, of a child brought into the house, labouring under the disease considerably advanced, small portions of the lower jaw came away, and the ulceration passed nearly through the cheek. I lost one child suffering under this disease, but it was much reduced by scrofula, when attacked, and its death might be said to have been hastened by, rather than attributed to it. The ulceration, at the death of the child, was less extensive than I have frequently seen it in other cases which terminated favourably.

In the commencement, the ulceration is of small extent, and there is commonly no sensible constitutional disorder. The child eats, exercises, and sleeps as usual, and seems to experience very little uneasiness. At this time, by a person not acquainted with its nature and tendency, it would be looked upon as a trifling complaint, requiring no particular attention. Such is the origin of a disease, which, if left to itself, soon becomes one of the most distressing and frightful of human maladies, and is sure to destroy the patient.

Those who have witnessed its progress to a fatal termination, describe its advanced stages as deplorable indeed. All the soft parts of the face are destroyed by the ulcerative process, leaving the cavity of the mouth exposed, and the bones naked and in a carious state. The fœtor becomes intolerable. In the course of the disease, a symptomatic fever supervenes, which assumes the typhoid character, and the patient dies in the most wretched and loathsome condition.

In order to form an idea of the remote causes of this singular affection, as occurring among the children of the Alms House, it may be proper to look at some of the peculiarities of the circumstances under which they are placed. Their diet is sufficiently nutritious, but coarse, difficult of digestion, and seldom varied. It consists of fresh beef, of which soup is always made; the soup serving for dinner one day, and the beef from which it was boiled, the next; rye bread fresh every day; indian meal porridge, a small quantity of molasses, and milk for their tea, which is bohea. In addition to these articles, they often have potatoes, turnips, cabbage, and in their season, other vegetables. Their drink, except the tea, is cold water. A different diet is allowed for the sick and debilitated, at the discretion of the physician: and wheat bread, rice, arrowroot, milk, chocolate, and beef, to be cooked according to his directions, are at his command.

Being kept constantly at school, put to bed early, and re-



quired to perform no labour, they have but little exercise. Their clothing is comfortable, and this, together with their persons, are kept as cleanly at least as is common among children of the poor. In the nurseries they are stripped and washed regularly once a week, and have their under clothing changed as often. One very striking circumstance attendant on the situation of the children, is, the very impure state of the air in which they constantly live. Confined in crowded school rooms during six hours of the day, and in their close and no less crowded lodging rooms through the night, prolonged by the practice of putting them to bed at the approach of the evening, there is but a little time that they have the opportunity of being in the open air, and even then, being restricted to the yard adjacent to the house, they breathe an atmosphere contaminated by the exhalation arising from the offals, excrements and filth of a thousand people of all ages, many of whom are utterly ignorant of, and will not be taught the comforts of cleanliness. No watchfulness on the part of the superintendant and officers of the place, can obviate the effects of passing filth on the air of the yard, and apartments; and notwithstanding continual scrubbing and washing, there is no time when it can be inhaled by one not much accustomed to it without disgust.

After stating the above facts, I shall hazard but little in ascribing the production of the disease, in the Alms House at least, in a great degree to the constant respiration of air contaminated in the manner above related; connected with habits of inactivity and slothfulness, necessarily arising from the want of inducements and opportunities to perform sufficient wholesome exercise.

From the manner in which the ulceration extends from the parts originally affected to those contiguous, as it commonly takes hold of the tongue and cheek where they lie in contact with the ulcers previously formed on the gums, there would seem to be some reason for considering it as

contagious. Yet I have not known or supposed it to be communicable. Its nature in this respect, might be easily ascertained by inoculation. It sometimes affects children debilitated by other diseases, during their progress, or after their termination. In such cases, I think I have found the ulcers to be more obstinate than in otherwise healthy subjects; at any rate, they form a very unfavourable prognosis with respect to the termination of the original disease. Several cases which supervened upon or immediately followed measles, have resulted fatally, within the last year, in the Alms House, (as I am informed.) I was lately called to a child in the lower part of the city, in an advanced stage of the measles, in which the sore mouth was making its appearance. The ulcers were distinctly marked, and though their character was changed, and their progress arrested, by the usual application, yet the child sunk under the original disease. Besides this case, I have seen two others in this city, which originated in private families. The first of these was brought into the Alms House, and recovered; the other, occurred in Augustus-Street, and was carried to the City Hospital, where I saw it in an advanced stage. This case I was told, terminated fatally. From these cases, and some others which have been mentioned to me, I should infer, that this disease must have been occasionally met with by most physicians, who have been for any considerable time extensively engaged in practice, in this city, especially by those who have been much in attendance on the poor. A medical friend writes to me from Philadelphia, that on inquiring of the physicians of the Alms House in that place, he could not learn that any such disease had been noticed there.

In Dr. Underwood's Treatise on the Diseases of Children, under the title of "a gangrenous erosion of the cheeks," he gives a very general account of a disease, which, in its advanced stages and termination, if it is not the same, bears a strong analogy to the one under conside-

ration. He states it to be of rare occurrence, and the description he gives, clearly evinces that he had not carefully observed its early stages.

"If neglected, or improperly treated," he observes, "the whole side of the face is often eaten away, together with the lip, so that the bare jaw-bone and inside of the mouth appear. In the end, the entire jaw-bone falls down on the breast, and the whole side of the face is dissolved in a putrid mass."

On this subject, Dr. U. refers to "a Treatise on Dentition, by Dr. Hurlock," printed in 1747, and a "Tract on the diseases of Lying-in Women, by Dr. Dean of Dublin," neither of which works have I been able to find.

On the subject of the treatment of this disease, I shall content myself with simply stating the method which I have universally pursued with very satisfactory success. What I consider by far the most important and indispensable part of duty, is the free application of undiluted nitric acid, to all the ulcerated surface. This remedy was introduced by Dr. Gilbert Smith, formerly attending physician of the Alms House ; and its happy effects in controlling, and rendering mild this otherwise dreadful and unmanageable disease, must ever be to him a source of the most pleasing reflection.

Previous to the application of the acid, if any of the teeth have become much loosened, they were taken out, and then, by means of a small piece of sponge, fastened to a wooden probe ; every part of the ulcerated surface was carefully touched with the acid. A solution of the sulphate of copper, six or eight grains to the ounce, was given to the nurse, with directions to wash the ulcers three or four times a day. In order to remove any sources of irritation which might exist in the stomach and bowels, an emetic and cathartic were usually prescribed on the first day of the treatment ; these were often combined, so as to make one dose answer both indications. The application of the



acid was repeated daily, and the vitriolic solution continued as above, until the peculiar appearance and fœtor of the ulcers were entirely removed ; this often happened in the course of the first twenty-four hours, and without a repetition of the acid ; but only in such cases as had existed but a short time, and made little progress when the treatment was commenced.

In good habits, and when commenced early in the disease, before the constitution had become much affected, the treatment above described usually arrested the course of the disease without difficulty, and effected a speedy cure. In a very large proportion of the cases which I treated, no other remedies were found necessary.

But when the ulceration has so far progressed as to give rise to much constitutional disorder, manifested by fever of a typhoid character, loss of appetite, diarrhœa, or much general debility ; or when it arises in depraved and weakly habits, or those reduced by other diseases ; other remedies are required.

Under these circumstances, it becomes necessary to use such means as will support and invigorate the system ; and as much as possible allay the irritation under which it suffers. In this state of the disease, analogy would lead us to the use of the various stimulant, tonic, and antiseptic remedies.

As I have treated few cases in which it became necessary to resort to any other remedies than the local applications and evacuants above described ; I can say but little from my own observation, with respect to the effects of this latter class of remedies in this disease. I have, in different instances, made use of wine, bark, yeast, and the nitric acid, and, I think, with all the benefit that under the existing circumstance could have been expected.

The ulcers have in a few instances retained their specific character, under the daily application of the acid and the solution of Sulph : Cupri. for considerable time without



making any perceptible progress, or producing much, if any constitutional derangement. In one case, an ulcer in the cheek continued stationary for two or three weeks, and the patient became feeble and emaciated, notwithstanding the diligent use of the usual remedies ; when on extracting two of the molar teeth, with which it had been continually in contact, the ulcer immediately healed, and the child soon recovered a perfect state of health. When this peculiar obstinacy arises from any local obstacles of this kind, the remedy is obvious ; when it proceeds from a feeble constitution or depraved habit, the peculiar circumstances of the case must point out the method most proper to be pursued.

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## REVIEW.

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*Elements of Medical Logic, illustrated by practical Proofs and Examples ; including a Statement of the evidence respecting the Contagious Nature of the Yellow Fever. By Gilbert Blane, Bart. F.R.S. London, 8vo. p. 219. 1819.*

ALTHOUGH it may be seriously questioned, whether the practice of dedicating books to those distinguished for their learning, does not, in general, spring more from the desire of recommending one's own production to the public, than from the disposition to bestow an expression of praise or gratitude on those who merit it; yet, in the present instance, we think Sir Gilbert Blane has very properly inscribed his "*Elements of Medical Logic*" to Sir Walter Farquhar and Dugald Stewart, inasmuch as these gentlemen, and the latter especially, have long stood pre-eminent for their scientific and *logical* attainments.

The introduction to this volume is replete with judicious reflections. As medicine has for its object the preservation and restoration of health, the author considers it as coming under the definition of an art, a term he says, the import of which consists in the adaptation of means to ends; and he thinks that the *faculty* by which this is accomplished, forms the best criterion for distinguishing rational beings from brutes.

He replies, by unanswerable arguments, to those persons of a sceptical turn of mind, who allege, that the powers and resources of nature, in the human, as in the brute creation, are all sufficient; that we do not possess such a power over the agencies of nature, nor such a knowledge of their ap-

plication, as to constitute an art; that the history of this pretended art in all ages, so teems with the fanciful influence of superstitious observances, the imaginary virtues of medicines, with nugatory, delusive, inefficient, and capricious practices, and fallacious and sophistical reasonings, as to render it little more than a chaos of error, or a tissue of deceit, unworthy of admission among the useful arts and liberal pursuits of man. The scepticism of such persons he regards as a disease of the mind, which, like some of those of the body, is the offspring of over refinement; and he observes, that, the great mass of error with which medicine has been encumbered, or as it were overlaid, has been engendered by an opposite malady of the mind, credulity.

The body of the work is divided into seven sections.—In the first section, the author enumerates the energies peculiar to animated nature; these he arranges in the following order, viz. 1. The Generative; 2. The Conservative; 3. The Temperative; 4. The Assimulative; 5. The Formative; 6. The Restorative; 7. The Motive; 8. The Sensitive; 9. The Sympathetic. This arrangement is evidently original, inasmuch as it is not founded on an enumeration of functions and organs, but on faculties pervading and belonging to the whole animal system; and the author conceives, if his enumeration be complete, that it ought to embrace all the principles of disease as well as of health, which can be brought under consideration; for if it includes the whole powers of life, all disease must consist in the disorder of one or more of them; and hence he suggests, that it might be made the basis both for a scheme of nosological arrangement, and plan of physiological instruction.

The second section is chiefly occupied in discussing the grounds upon which theoretical investigations in medicine should be conducted. After adverting to the sound principles of philosophizing, adopted by Democritus and his followers, of whom Hippocrates was the greatest; and to the syllogistic logic of Aristotle, which for more than a thou-

sand years usurped the place of all other literary and scientific pursuits, he justly observes, that it is to Bacon the world is indebted for dissipating the clouds of false philosophy, and pointing out the road which led to solid learning, and the discovery of interesting truths.

The rest of the volume is devoted to the consideration of the obstacles which have obstructed and retarded the progress of medical science; and these form the subjects of the following sections, viz. 3d. The great diversity observable in the constitution of individuals; 4th. The difficulty of appreciating the efforts of nature, and of discriminating them from those of art; 5th. Superstition; 6th. The ambiguity of language; 7th, The fallacy of testimony.

From the above syllabus our readers will perceive, that the object of Sir Gilbert Blane is to bring into one view whatever relates to medical dialectics. In regard to the manner in which he has executed his intention, we are free to observe, that, in general, his pages are remarkable for great perspicuity, chasteness of style, and occasional originality of sentiment. There is one section, however, which, as it principally relates to the important subject of Yellow Fever, and is peculiarly interesting to American physicians, we deem it our duty to examine with more than ordinary attention. We allude to the 7th section, or that entitled "THE AMBIGUITY OF LANGUAGE." The author introduces it with the following observations:—

"As the end of language is the communication of thought, it is self-evident that there can be no such thing as correct reasoning, unless the same import be annexed to the same words in the oral and written intercourse of mankind. A large proportion of all the false reasoning and controversy, which has existed among the learned and unlearned of all ages, has arisen from the want of a precise definition of words. The most valuable parts of the writings of Locke, are those which relate to the abuse of language. It is a subject upon which there is great room to dilate; for none



of the departments of practical knowledge have suffered more than medicine, from ambiguous phrases and verbal disputations. But this dissertation having already drawn out to a great length, the author will satisfy himself with illustrating the subject with a very few examples, referring the reader to the chapters in Mr. Locke's work, which relate to this subject.'

It is doubtless yet impressed on the mind of the reader, that Sir Gilbert Blane professes to write a treatise on the elements of medical Logic; but that impression is soon to be weakened if not entirely obliterated; for notwithstanding the ambiguity of language is a subject upon which there is great room to dilate, the author is about to abandon the didactic method of an elementary writer, and to assume the imposing manner of a partisan, suddenly roused to action upon the mention of a subject in which his feelings, his honour, and his fame are deeply interested. If none of the departments of practical knowledge have suffered more than medicine from ambiguous phrases and verbal disputations, was it not to be expected that he would have treated the subject somewhat in extenso, instead of referring to the chapter of Mr. Locke. But in apology for this omission, we are told, that as his dissertation has already drawn out to great length, (128 pages,) he will satisfy himself with illustrating the subject by a very few examples. These are *very few* indeed; viz. scurvy and yellow fever. On the former he bestows a single paragraph and in that does not attempt to settle definitively the meaning of the term; but on the latter, apparently forgetting the length of his dissertation, he dilates over seventy pages, (including a note,) leaving nothing untouched which relates to the matter in controversy.

Much as we venerate this accomplished physician and elegant scholar, we cannot avoid remarking, that by entering into an elaborate discussion on the nature and origin of yellow fever, he has not only departed from his province as a writer on the elements of medical logic, but that in debating

this subject, he has failed to exhibit an example of logical reasoning. It is true, he tells us in his title page, that his work contains "a statement of the evidence respecting the contagious nature of yellow fever ;" and from this information we were induced to expect a simple but lucid enumeration of the facts and circumstances which tend to prove the contagious character of that disease. Such a course would have come within the pale of medical logic : it seems however, that the author, over indulgent to his feelings, seized the passing opportunity to declaim with illogical vehemence in defence of opinions he had formerly promulgated.

It is remarkable that the author uses throughout this section the terms infection and contagion as synonymous, without adverting to the distinction made between them as early as 1796, by Dr. Richard Bailey of New-York, "whose views on this subject" says an able writer, "have been widely circulated in his own country and in England, and procured for him much reputation, though unfairly assumed as his own by Dr. Adams of London."\* To the want of established and well-defined terms, in this department of science Dr. Hosack attributes consequences which every philosopher must sincerely deplore. "The abuse of the terms contagion and infection," says he "and the neglect of writers, in not annexing to them a precise definition of the manner in which they severally employ them, have, I believe, been the source of our medical warfare relative to the contagiousness of yellow fever and some other diseases."† A further proof of the necessity of settling the import of these terms, is afforded by the singular definition given of them by Dr. Parr in his Medical Dictionary. (vide Contagio.) He remarks, "We should approach more nearly to common language if we employ the adjective *infectious*," to disease communicated by contact; for we *infect* a lancet, and we *catch* a fever by con-

\* Edinburgh Encyclopedia, vol. xii. p. 750.

† American Medical and Philos. Register, vol. 2. p. 14.

tagion;" unhappily, too, we sometimes catch a cold! "But, he continues, "in the present state of our knowledge of the subject, we must employ these terms as synonymous, *though we shall in general follow the distinction now suggested.*"

Although Sir Gilbert Blane uses the terms infection and contagion indiscriminately to express a specific poison generated by disease, he nevertheless occasionally employs the former to signify concentrated miasma, the production of crowding and domestic filth, apparently without perceiving that he thereby renders it ambiguous and vaguely complex. The omission to notice the indeterminate application of these words, and to apprize us of the manner in which he intended to use them, would not have appeared so very important and deserving of criticism, were it not that the author is treating of the ambiguity of language. In the progress of our remarks we, also, shall consider them, though reluctantly, as synonymous.

'In order to treat this subject (yellow fever) intelligibly,' says the author, "it is necessary to remind the reader, that there are three sorts of remote causes which give occasion to fevers in whatever climate they may arise. One class of these causes is the exhalations of the soil, producing intermittent and remittent fevers, which occasionally pass into continued fevers, particularly when under the influence of other remote causes."

The second class of occasional causes, is vitiated human *effluvia*, generated by the living body, under circumstances of crowding, filth, want of ventilation, and change of apparel, aggravated occasionally by scanty and unwholesome food, as exemplified in the jail, hospital and ship fever, and that of the indigent part of the population: and all pestilential epidemics seem to have had a similar origin, diversified according to circumstances not always definable and ascertainable." (p. 130.)

The third class consists of that disturbance of the system occasioned by fatigue, insulations, intemperance, the pri-

vation of food and sleep, sudden alterations of heat and cold, acting either jointly or singly, in creating fever."

Of these, the second only is found to be contagious. In this class the author places yellow fever. "They are all three found to exist in the West Indies, (by which is meant the islands called the great and small Antilles, or Caribbean and Mexican, Archipelago, and the adjacent coast of America,) in common with other countries." But he remarks, that at this station the fevers from these three remote causes are all accompanied with a yellow colour of the skin, which being a conspicuous symptom, has procured for them all the appellation of yellow fever, giving occasion to great confusion and serious mistakes. (p. 133.)

"In order to give that precision to language which is necessary on every subject, and with a view to avoid misconception and wrangling, these three classes of fevers shall be designated as follows; the first shall, in the course of this discussion, be called the endemic; the second, the pestilential or malignant epidemic, or typhus icterodes, as it is termed by some systematic writers; the third shall be called the sporadic." (p. 136.)

Without calling in question the author's claim to originality in the above classification of causes, we have pleasure in observing, that, if *priority* in originating and proclaiming improvements in science, give honour and distinction to character, then, these attributes belong exclusively to our countryman, the late Dr. Edward Miller of New-York; for, omitting the third class of Sir Gilbert, which is, for the most part, founded on incidental circumstances, and therefore comparatively unimportant, his first two classes agree with the arrangement of the remote causes of certain febrile and pestilential diseases, proposed by the former gentleman in his admirable nomenclature of fevers, published in 1794; the first class of Sir Gilbert, corresponding with his *Atmosphera koino miasmatica*, and the second, *Atmosphera ideo miasmatica*. But though we



cheerfully subscribe to the author's classification of *causes*, yet from his subsequent views we entirely dissent: we think he has greatly mistaken the influence and effects of the causes he has enumerated, and we feel it incumbent on us to expose his errors.

If the exhalations of the soil can produce intermittent and remittent fevers, which under the influence of other remote causes, become continued; we ask why may not the same influences still more extended, produce an epidemic yellow fever. To stop at simple continued fever, is to stop without a reason: the progression yet continues easy and natural, and obviously leads to results satisfactory and conclusive. Koino miasmatic exhalations present themselves under various modifications, and possess diversified powers of virulence. Sometimes the diseases arising from them are mild and tractable, at other times violent and fatal; but in all, there is such a close affinity or similitude of feature as to evince they are members of the same family. It is a fact, about which there is no dispute, that miasmata vary in degrees of force in various seasons, and at different places in the same season. These considerations not only invite, but irresistibly draw us to the belief, that *epidemic yellow fever* is a disease produced by the exhalations of the soil, modified and exalted in virulence by the influence of other remote causes. In this view of the subject we regard it as holding a higher place in the scale of miasmatic fevers than intermittents and remittents. If it be demanded, what are the remote causes which perform so important a part in the production of yellow fever? we answer, a long continued and high degree of solar heat, humidity, a peculiar constitution of the atmosphere, a crowded population, and public neglect of cleanliness.

Exhalations from animal and vegetable matters, in ordinary seasons produce febrile diseases of the milder forms; but when those effluvia are generated, under a high range of thermometrical heat, and are blended with the influence

of other remote causes, these diseases assume a more violent character, rising in malignity, till they reach the grades of yellow fever and plague. It is impossible to determine the relative power which each cause severally exerts in the production of yellow fever. Solar heat is doubtless the principal agent in generating the deleterious miasm, and in predisposing the body to be affected by it, while the others perform their part, either by directly favouring the formation of exhalations, or rendering the system more susceptible to their operation. But besides these causes, there are others which frequently have an immediate agency in exciting the disease, as intemperance, fatigue, fear, insolation, the privation of food and sleep, and sudden alterations of temperature. That all the circumstances above-mentioned, are necessary to the production of typhus icterodes, we will not affirm; some of the more adventitious ones may be entirely wanting, but their absence is sufficiently compensated by the peculiar concurrence and greater intensity of those which exist. Persuaded of the truth of the preceding remarks, we think the author has grossly erred in referring yellow fever exclusively to his second class of occasional causes. But fully intent on establishing his doctrine, he observes:—

“The fevers proceeding from long confined human effluvia, seldom originate in the West Indies; for the heat of the atmosphere is such, that it is not necessary to exclude the fresh air, as in Europe, and other temperate climates. Such fevers, therefore, have become *epidemic there, only in consequence of infection occasionally generated and imported by ships under peculiar circumstances of crowding and filth, exasperated by the length of the voyage and various hardships, affecting the minds of the crews and passengers.*”  
(p. 138.)

These data lead directly to the conclusion that yellow fever is *ship fever*, propagated by contagion; and, if this be true, we think, that in denominating this disease, he should

have adopted the latter term, inasmuch as it would have removed all ambiguity, and possessed the peculiar advantage of expressing the origin of the disease. But that ship fever is capable of spreading its ravages by contagion through the islands of the Antilles, and to the shores of America and Europe, is, at the present day, an opinion so antiquated and absurd, as scarcely to deserve our serious consideration. And yet such is the unavoidable inference from the author's whole train of reasoning. Now it must be obvious to every reader, that in using the term "ship fever" to exemplify his second class of occasional causes, he intends to designate a disease different from typhus icterodes; but what distinction there is between them, we are not informed, nor can we conceive, since we are explicitly told that both are contagious and have an identity of origin. How far such views are calculated to correct the *ambiguity of language*, we leave for the writers on logic to determine. In a preceeding page, (135,) it is said:—

"Another remarkable circumstance with regard to the origin of the West India fevers, is, that they sometimes are found to arise from the foul vapours of ships, replete with filth from long neglect of cleanliness. And it is remarkable, that the fevers arising from this cause are found *sometimes to be contagious, and sometimes not*, according to the nature and intensity of the effluvia, and the susceptibility of the subjects exposed to it."

The advocates of contagion may here take a bold and determined position; and with this twofold argument, defend themselves at every point. A ship arrives in port, replete with filth and foul vapours, which produce fever;—is typhus icterodes become epidemic? that fever is contagious; but if no epidemic occur, it is non-contagious. This is truly a most accommodating theory, and it enables its supporters to explain with wondrous facility the mysterious origin and extension of yellow fever; especially as they arrogate to themselves the sagacity to detect what kind of fever



is contagious, and what is not. And hence, too, on the occurrence of epidemic yellow fever in the West Indies, United States, or south of Europe, they are seldom or never at a loss to designate the ship in which the pestilence was imported. Besides, "it forms no objection to the importation of a fever being referred to a particular ship, that the fever did not actually exist on board of her at her arrival." (p. 163.) No; because the crew and passengers have become habituated to the poisonous effluvia on board, which immediately infects visitors on her arrival, who subsequently propagate the disease by contagion.

That fevers of various forms and different degrees of malignity do frequently originate on ship-board from filth and other causes, and that visitors to ships in which the crews and passengers are in good health, may contract a malignant disease, are facts so well ascertained as to demand universal credence. But the manner in which they are here perverted in order to explain the origin of our pestilential epidemics, if not already obvious, will fully appear in the sequel.

The author having discussed the proofs in favour of the foreign origin and contagious nature of yellow fever, proceeds to what he calls, the matters of fact which can be adduced on the same side of the question. The more important of these we shall notice in the order they are given.

1st. "It has never shown itself in the first instance, but in a sea-port town, and never in the interior of the country, whether island or continent."

This declaration is undoubtedly founded on the statements of those whose opportunities of seeing the disease have been limited to sea-ports; and we confess that in so far as the salubrity of Great Britain is in danger by her ships importing yellow fever, such a declaration may reach the truth near enough to satisfy the people of that country; but in the republic of North America, where this terrible form of pestilence is annually experienced in its greatest



malignity, and where motives, as powerful as the love of life, are prompting physicians to a diligent and patient research into its nature, it would argue a superstitious reverence for foreign authority, were we to yield a tacit assent to the assertions of Sir Gilbert Blane, whilst the means of their refutation are abundantly spread before us.

The writings of Dr. Miller, with which this author professes to be acquainted, and the pages of the Medical Repository, furnish facts, which prove conclusively that the yellow fever has occasionally appeared in the interior of our country. In order to sustain the character of an impartial logician, he should have impugned these facts, and shown the incompetency of their observers. But however unworthy such proofs were of his notice and candid examination, we shall adduce them as correctives of his "matters of fact."

"The venerable Dr. Anthon of this city," says Dr. Miller, "whose accurate acquaintance with the pestilential epidemics of New-York, enables him to decide in the most satisfactory manner, assures me that he has often seen the disease in the interior country, and particularly in the low lands near the river Illinois, after an extensive inundation of that river, succeeded by warm weather." Mr. Volney, the celebrated traveller, found yellow fever, which he accurately describes, in the western regions of the United States; and Mr. Ellicott, in his voyage down the Ohio, explicitly states, that many of the inhabitants of a village a few miles below the Great Kanhaway and Ohio Rivers, fell victims to that disease.\* Dr. Watkins, also, presents himself

\* We are told by Dr. Hosack, in his "observations on febrile contagion," &c. that Mr. Ellicott, before his death, renounced the opinion, that the disease mentioned in his journal was yellow fever. It is said he considered it as nothing more than the ordinary bilious fever. The representation of this writer so well agrees with the other authorities mentioned in the text, that, notwithstanding his change of opinion, the disorder as described by him, must still be regarded as that grade of fever usually called yellow fever. The following are his words. "Many of the inhabitants this season fell victims

as an unexceptionable witness, having seen the disease in Philadelphia, and declares, that in 1797, he resided in the village of *New Design*, about fifteen miles from the Mississippi and twenty miles from St. Louis, and that the yellow fever destroyed fifty-seven of the inhabitants, or more than a fourth of the whole population.\* Dr. Miller states, and doubtless on the best authority, that "in some of the more exposed situations, and after very hot and damp summers, the yellow fever often assumes an epidemic character in the country. The malignant disease at Catskill in this state, in the year 1803, (see *Medical Repository*, vol. 8. p. 105,) affords an instance of this kind. In the year 1793, it prevailed in many parts of the country in the eastern, middle, and southern states, where no suspicion of contagion could exist."† Dr. Haygarth, a professed contagionist, in speaking of Dr. Fellows, a gentleman enlisted on the same side, says, that in his journey to Malaga, "he obtained authentic information from the chief magistrates and physicians, that the ravages of this pestilence were not confined to places connected with tides, as is said to be the case in America; but that it was fatal to hundreds in several inland towns of Spain."‡ This fact so well corresponds with similar occurrences in the United States, in which there is not the slightest reason to suspect contagion, that we cannot for a moment hesitate in concluding that the disease was indigenous in the interior of Spain. The testimonies of the gentlemen above mentioned, to use the language of our author, as applied to Mr. Pym, Sir Joseph

to the yellow fever. *The mortal cases were generally attended with the black vomiting.* This disease certainly originated in the town, and in all probability, from the filthiness of the inhabitants, added to an unusual quantity of animal and vegetable putrefaction, in a number of small ponds and marshes within the village." Vide *Journal of a voyage, &c.*

\* *Medical Repository*, vol. 4. p. 74.

† *Miller's Works*, p. 142.

‡ Haygarth's letter, as quoted in *Med. and Philo. Register*, vol. 1. p. 85.

Gilpin, Don George Juan, and Don Antonio D'Ulloa, "cannot be invalidated without impeaching the moral character of these men; a species of argument however, which it is to be deplored has not in every instance been abstained from by the partisans of this question."

2dly. "It has in most instances been ascertained, that it has made its appearance in that sea-port after the arrival of one or more ships, either under those peculiar circumstances which engender infection, or conveying the infection from ports where it has already existed. Most of the great epidemics of the West-Indies, North America, or Europe, can be traced to one or other of these sources. If it cannot in every instance be traced, this may happen from the want of historical facts; neither does it follow that infection does not exist, though it cannot be traced, for nothing is so subtle as infectious effluvia."

We have already shown, in part, to what feeble arguments the contagionists resort, in order to support their position. We are now presented with others equally remarkable. When the author states that it has in *most* instances been ascertained that the yellow fever has made its appearance after the arrival of one or more ships having infection on board; he clearly admits that the contagionists themselves are sometimes unable, even with the help of their accommodating rationale, to point out the ship or ships in which the contagion was imported. This circumstance, however, it seems, forms no objection to their doctrine, for, "if it cannot in every instance be traced, this may happen from the want of historical facts." *Aye, there's the rub.* Does he mean that the arrival of ships in a filthy state, or from a tropical climate, cannot always be known? For of a truth, none but smugglers can elude the officers of the municipality and customs; arrivals are always registered, and no ship can enter a harbour in a condition so foul as to engender pestilence without giving direct and fatal notice of her presence. But there is yet another circumstance urged



in explanation of the same difficulty, viz. "neither does it follow that infection does not exist, though it cannot be traced, for nothing is so subtle as infectious effluvia;" and the author illustrates his idea by observing, that "it is well known that small pox and measles find their way where there is no possibility of tracing the source of the infectious matter." If he intends by this to consider the contagion of yellow fever as analogous to that of small pox and measles, he evidently avails himself of a mode of reasoning which he has expressly discarded. "There is here" he says, "the utmost risk in reasoning from analogy; and no inferences can be trusted, but those which consist in those pure matters of fact which constitute each particular case." (Page 132.) There is in fact no analogy between these diseases. If yellow fever, like small pox, were contagious, it would spread from sea-ports to the adjacent and interior countries; it would extend its pestilential ravages over states, and empires, and continents.

3dly. "No part of the population of the towns where it has broke out has been affected, but such as had communication with shipping, directly or indirectly."

A majority of the population of every sea-port may be said to have a direct or *indirect* communication with the shipping; and hence the contagionists often display their ingenuity in tracing the epidemic to some ship, sailor, visiter on ship-board, article of clothing, or merchandise. The numerous instances in the Atlantic towns of the United States, where persons have sickened with yellow fever, at the commencement of an epidemic, without any other than the remotest connexion with ships, is powerfully adverse to the idea of contagion. And the fact, that those who visit the streets, of an infected district, contract the disease, months after the inhabitants have fled and the shipping removed, is proof irrefragable, that the whole story of yellow fever contagion is a stupendous fiction, got up in an age of inexperience and superstition, and unfortunately



perpetuated by authorities too high to be questioned. Who will believe that a ship arriving in port, freighted with small pox, a poison more certainly communicable than any other, would be capable of so infecting the streets of a city, as to produce the disease in a transient visiter at the distance of several hundred yards from the wharf where she lay only a few days, and that too, at a period of two months after she was removed, and the people had abandoned their homes.

Nevertheless, it is true that ships may import the seeds of pestilence, and thereby produce disease in those who have a direct communication with them. Indeed, the population living in the immediate vicinity of their moorings, may experience the deadly effects of the pestiferous vapours thus introduced. Instances of this kind could be unequivocally designated; and we believe that if such vessels were permitted to retain their situations, they would continue to scatter sickness and death among those who ventured to approach them. But such facts by no means prove either that the disease is contagious, or that our pestilential epidemics have originated from a foreign source. In every instance the fever arising from ships under the circumstances mentioned, has been limited to a few individuals, and has totally disappeared on removing them to a distance: whereas our epidemics have had their rise, progress, and decline, like all other epidemics, without being influenced in any of their stages by the arrival of ships from any quarter of the globe. It would be remarkable, considering the extended commercial intercourse of the United States with foreign ports, if a coincidence should not sometimes happen between the arrival of infected ships, and the occurrence of epidemic yellow fever. But such an event, alone, would no more evince that the epidemic grew out of an exotic germ, than that the unusual heat and filth which engender an impure atmosphere, (the medium of communication or *sine qua non* of some

contagionists,) were imported from the torrid regions of the south.

Nor can it be denied that the places in which yellow fever chiefly exerts its desolating ravages, have a maritime situation; the crowded ports are generally the places in which it makes its appearance. It is doubtless from these circumstances the delusion has arisen, that the disease is of foreign origin; and we grant that, formerly, on minds ignorant, and under the influence of fear, they were calculated to produce that impression; but in the present enlightened age, when experience and philosophy have extended our knowledge of diseases, and enlarged our powers of inference, we are surprised it should continue to keep its hold on the mind. Our limits at this time will not admit of a full discussion of this interesting subject; we shall therefore advert to a few only, of those considerations which might be advanced in elucidation of the fact, that yellow fever generally appears on the sea-board.

It will be conceded, that to the ordinary impurities of all large cities, there are superadded in sea-ports many of a peculiar and adventitious character. These combined and acted upon by a high degree of solar heat, necessarily produce exhalations more deleterious in their nature, than those which arise from a less compound source. Those cities have generally suffered the most, which have an alluvial situation, and are surrounded by swamps, savannas, and a campaign country, all of which are favourable to the prevalence of miasmatic fevers. In the exceptions to this observation, excessive heat, filth, crowding, &c., have formed a sufficiently operative source of disease. Epidemic constitutions of the atmosphere are commonly felt in their greatest effect in a parallel along the coast, and consequently, the summer and autumnal fevers usually exhibit in the interior a milder form, than in situations bordering on the sea. There is something in the mixture of the land and sea airs, which is almost proverbially hostile to the in-

tegrity of health. This truth, in the temperate latitudes, is exemplified, by the incomparably greater prevalence of consumption, scrofula, croup, and some other diseases in maritime districts, than in countries remote from the sea. Although these complaints, for the most part, appear in colder seasons of the year, still it is probable, that the air on the borders of oceans and seas constantly retains a peculiarity, which conjoined with heat in the summer and autumn, predisposes the body to febrile diseases, and heightens the epidemic influence of the atmosphere.

The author specifies three instances in evidence of the communication of yellow fever, by contagion, from one ship to another. In a letter to Mr. Rufus King, minister from the States of America, and another to Baron Jacobi, minister from Prussia, he says, he "laid particular stress on what occurred regarding a French ship taken in battle on the coast of America, in May, 1795, on board of which this fever or its infection was found, and was communicated to the seamen of the British ship, by the men in health, who were shifted into her from the prize." He observes, that the facts of this case were so speciously contested by Dr. Bancroft, as greatly to frustrate the impressive effect which his statement was calculated to produce; and he adds, that the reader will be able to judge of the solidity of his objections, from a note at the end of the volume. But admitting the facts of this, and the other instances which are of a like description, to be correctly stated, we cannot assent to the conclusion he has drawn from them. The men of the infected vessels doubtless carried with them to the ship their infected clothes, chests, &c., which had been, probably for a long time, steeped in the miasma of pestilence. Under these circumstances who can doubt but that the disease may be communicated, and that too, without the intervention of a specific contagion. In full health, and highly susceptible of noxious impressions, the healthy crews were brought in contact



with persons and their baggage direct from the forecastles of ships teeming with deadly exhalations. Miasmata, if concentrated, may be retained in clothing for a season, as well as the virus of small pox or measles, and in this way we unhesitatingly believe that yellow fever has in some instances been communicated.

The author examines at considerable length the grounds upon which the "mischievous delusion is founded," that the yellow fever is not contagious, and *differs only in grade from endemic bilious fevers*. Of these we select the following, which are the principal.

"The great similarity of this pestilential epidemic to the endemic and sporadic fevers of the Antilles. The most conspicuous point of resemblance is the yellow colour of the skin. The resemblance, however, is not perfect here; for the colour of the former is a dingy orange; in the other two, a bright yellow." (p. 164.)

"There is another symptom in which all the three species bear a resemblance to each other; *the vomiting of a coffee-coloured liquid* in the dangerous and almost hopeless stage. This symptom, however, is far more frequent in the pestilential epidemic, than the other two." (p. 165.)

"But admitting the symptoms to be ever so similar, it does not follow they are identical. A great proportion of the cases of the true plague were without the tokens or diagnostic characters, and some could not be distinguished from a continued fever; but in spite of this close resemblance, they are *essentially* different in their nature." (p. 169.)

In the first part of the section under review, he says, the yellow colour of the skin is not unknown in other parts of the world, even in cool or temperate climates; "but it is only in the West Indies that it is met with as a general and characteristic symptom of endemic and epidemic fevers of the country."

We candidly acknowledge that it is on such grounds we have founded the opinion, that the endemic and epidemic



fevers of the West-Indies, and we may add of the United States, are essentially alike in their nature, though different in grade. And with sincerity do we believe, that the logic by which the author attempts to prove them otherwise, is not only false, but calculated to bewilder those liberal and ingenuous youth, who, passing from academic to professional pursuits, cannot but immediately perceive, that if such reasoning be adopted in medical inquiries, then truly must the science of medicine consist of abstract notions and incomprehensible doctrines. Their lessons from Bacon and Locke, from Stewart and Watts, they now discover can serve them no purpose, since medical logic declares, that if diseases agree in every essential particular, and "symptoms be ever so similar, it does not follow they are identical." According to the principles of Sir Gilbert, the ordinary rules of generalization must be dispensed with, and those things which agree together must not be arranged in the same genus or species. But such logic we entirely disclaim; and although we are aware of the thousand circumstances which render medical disquisitions abstruse and delusive, still we can with confidence assure the student, that in medicine the same rules of induction are employed in the discovery of truth, that were taught him in the prelections of his alma mater.

The author has given us a glowing picture of the epidemic in its most aggravated form. As a specimen of his masterly talent of delineation, we select the following:—"The characters of this fever, as distinguished from the ordinary fever of the climate, are depicted in lively colours by the surgeon of the *Eurus* frigate, in his journal, examined officially by me in 1797. In a few days after arriving at Grenada, eighty men were rendered unfit for duty, that is, about a third part of the crew; the eyes swam as it were in blood, with excruciating pain, chiefly in one eye; the eye-ball started in one: in several cases, one of the eye-balls was absorbed: in the course of convalescence

some lost their sight, though they retained the substance of the eye. Who will say that this differs in nothing from the endemic and sporadic fevers of the Carribean Islands?"

We certainly do not wish to be classed with those who cannot see palpable discrepancies where they actually exist; nor, on the other hand, are we desirous of being considered so destitute of judgment, as to presume that the several grades of a disease are several specific disorders. Our senses, we think, enable us to perceive distinctly, that the yellow fever often materially differs from common bilious remittents; they also enable us to perceive a marked difference between the mild aspect of variola discreta, and the terrific features of variola confluens; or between scarlatina mitis, and scarlatina maligna, or between typhus mitior, and typhus gravior, notwithstanding observation and reason have long since taught us that they are modifications of the same disorder. In order to ascertain whether two forms of disease, the one mild, and the other malignant, are essentially the same, we must carefully note their several varieties. If these constitute a series which unite the two, we may safely conclude that they are similar in their nature. This mode of induction enables the physician to settle the identity of the mild and malignant scarlatina, and also of the endemic and epidemic fevers of the Carribean Islands.

With a view to illustrate and enforce his argument, the author emphatically asks, "has it not occurred to every practitioner in this country to see sporadic fevers, when there was not the least reason to suspect contagion, so far resemble the typhus fever from infection, as to be undistinguishable?" Can this learned writer suppose that his readers are prepared to believe that two things which so nearly resemble each other, as to be undistinguishable by common sense or philosophical acumen, are not identical in kind? Can he suppose them stupid enough to believe that those things are not alike which are alike? Our author it seems endea-

vours to establish a distinction between the undistinguishable fevers of Great Britain, not by pointing out their diagnostics, but by referring to a difference in their causes; and in like manner he would found a distinction between the endemic and pestilential fevers of the tropics. We are not inclined to dispute that fevers may arise from a variety of sources; so far from it, we are convinced of the fact; but when yellow fever contagion is insisted on as one of these sources, and we are told that the disease arising from it is undistinguishable, and at the same time entirely different in its nature from that induced by well-known causes, we are compelled to pronounce it an instance of the sophism *non causa pro causa*.

There is one more argument, which from its plausibility, and the strenuous manner in which it is urged, demands a little of our attention. In proof of the distinct and contagious character of yellow fever, the author affirms, that it is, "never or very rarely, capable of being caught a second time." Until recently, few or no contagionists have had the boldness to advance this idea in the form of a dogma. It is now said, that its truth is amply tested by the repeated and innocent exposure of many persons to contagion after they have once passed through the disease. Without enlarging this article, by adducing facts to expose this *fallacia accidentis*, it will be sufficient to observe, that the human constitution has the power of accommodating itself to the circumstances in which it is placed. Hence the West Indian, accustomed to a vertical sun and an atmosphere envenomed with miasmata, seldom exhibits in his febrile attacks, during an epidemic, the aggravated symptoms of yellow fever. His disease assumes a milder or remittent type; whereas, the same causes operating on the newly arrived and highly susceptible European, produce a disease of the most malignant character; still the attacks of both however unlike in phenomena, as much belong to the same kind of fever, as the hilarity of a veteran bacchanalian and the stupor of a disciple of Mahomet after a carousal over



wines, belong to febris temulentia: the secondary occurrence of epidemic fever, therefore, in a mild form in the West Indian or assimilated European, is virtually a secondary attack of yellow fever. Besides, it is well known that a removal from a tropical to a northern climate, restores the susceptibility of the system, and renders it again subject to the destructive agency of that fearful scourge.

We should here close our remarks on this much controverted subject, were it not that the author has referred us to an extremely judicious arrangement of the varieties of contagion, by Professor Hosack of New-York. (American Medical and Philosophical Register, vol. 2. p. 14.) As the opinions of this gentleman have been widely circulated, and have received the approbation of several distinguished physicians, we shall solicit the indulgence of our readers, while we briefly review the grounds upon which they rest.

Professor Hosack arranges the varieties of contagion into three classes. The first class embraces all those disorders which are communicable exclusively by contact. The second includes all such as are communicable both by contact, and through the medium of a pure or impure atmosphere. In the third class, which is the one we purpose to examine, he places plague, yellow fever, typhus *jail, ship, hospital, or lake fever*, and dysentery.

"These diseases are only," he says, "in general communicable through the medium of an *impure* atmosphere; in a pure air, in large and well ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges immediately removed, and, attention paid to cleanliness in general, these diseases are not communicated, or very rarely so, from one to another. But in an *impure* air, rendered so by the decomposition of animal and vegetable substances, as takes place in low marshy countries, or by concentrated human effluvia, as in camps, jails, hospitals, or on ship-board, they are rendered not only extremely malignant and mortal in themselves, but



become communicable to others, who approach the sick, or breathe the same atmosphere which has become assimilated to the poison introduced, insomuch that the same specific disease is communicated, whether it be the plague, yellow fever, typhus, or dysentery. Hence we account for the fact stated by Sydenham and other writers on epidemics, that the prevailing disease swallows up all other disorders; i. e. that during the prevalence of an epidemic plague, typhus, dysentery, and other diseases of this class, every indisposition of a febrile sort readily assumes the character of the prevailing disorder. We know this to be experienced in the diseases of other countries, and we see it daily exemplified in our own. Both in our cities and in the country towns, when after heavy showers of rain, and the action of a hot sun, a decomposition of vegetable and animal substances takes place, and dysentery or typhus fever is produced, it assimilates the air to itself, whatever be the acting poison."\*

From these singular views, we distinctly understand that the effluvia arising from animal and vegetable decomposition, are capable of producing sporadic plague, yellow fever, typhus, or dysentery; but that the same cause is not adequate to the production of an epidemic, until the sporadic cases have

\* As Dr. Hosack has long plumed himself on the originality of his theory of febrile contagion, we think it proper to remind our readers, that it is only a modification and extension of Sydenham's views relative to the causes of the plague. The truth of this remark will appear from the following quotation. 'But besides the constitution of the air,' says Sydenham, 'as a more general cause, there must be another previous circumstance to produce the plague, viz. the receiving the effluvia or *seminium* from an infected person, either immediately by contact, or mediately by pestilential matter, conveyed from some other place. And when this happens in such a constitution as we, have mentioned above, (pestilential,) the whole air of that tract of land is quickly infected with the plague, (assimilated says Dr. Hosack, to the poison introduced,) by means of the breath of the diseased, and the steam or vapour arising from the dead bodies, so as to render the way of propagating this dreadful disease by infection entirely unnecessary: for though a person be most cautiously removed from the infected, yet the air received in by breathing, will of itself be sufficient to infect him, provided his system be disposed to receive the infection.

imparted to the air a specific virus, which assimilates the impurities of the air into a poison like itself. Agreeably to this idea, we know not why intermittents and remittents should not be considered as contagious as yellow fever or dysentery; for they arise, first sporadically, and then prevail epidemically in every country which affords exhalations of the soil.

We are not told whether the contagious principles of small pox and measles have the power to convert impure air into genuine variolous and morbillous infections. If these, active as they are, have not assimilating properties analogous to those which are said to belong to the contagions of plague, yellow fever, typhus, and dysentery, we may rest assured that the *idea* of specific ærial fermentations is itself a mere ærial conceit.

But it is said that the communication of yellow fever and its associates is governed by a particular law. Has it been discovered by chemical research that there are different laws of gaseous fermentation? one by which the ordinary emanations from animal bodies are resolved into their elements, and thus prepared to form new compounds; and others by which the varieties of febrile contagion convert *impure* air into their own nature? If the *specific* poisons of plague, yellow fever, typhus and dysentery, possess assimilating qualities, by what law of chemistry are the infectious principles of small pox, measles, chicken pox, hooping-cough, and scarlet fever deprived of those qualities? It is absurd to suppose, that certain febrile disorders, whose contagious character, to say the most, is doubtful, have the power to extend themselves by assimilating foul air to their respective poisons, when it is demonstrable, that other febrile complaints, on all hands admitted to be contagious, have no such power. Is it conceivable that a principle, *sui generis*, emanating from the system and mixed with impure air, that is, *dead matter*, can, by any exertion of chemical affinities, procreate and multiply a substance, which for its original generation

required the wonderful organic or secerning operations of the human body ?

The Professor's doctrine assails the well known truth, that the tendency of all the excretions and secretions, (the latter including specific contagions,) when removed from the body, is, to lose their peculiar characters, and to undergo the process of decomposition. Some of these retain their properties longer than others, but still they constantly tend to decomposition; and consequently, so far from believing that any of them can assimilate the impurities of the air to their own nature, we are convinced they pursue an opposite course, and eventually become themselves reduced to mere putrescent and noxious matters. The infectious materials of small pox, measles, and all other contagious disorders, are by no means exempt from the operation of this universal law.

'Hence we account' the Professor's explanation of 'the fact stated by Sydenham and other writers on epidemics, that the prevailing disease swallows up all other disorders,' as one of those speculative follies into which great and intelligent minds are sometimes betrayed. The nature of those qualities of the atmosphere which constitute its power of assimilating diseases, is undoubtedly placed beyond the scrutiny of the human understanding. We know, however, that one condition of it produces plague, another yellow fever, and another dysentery, &c. But unfortunately for the theory of Dr. Hosack, the assimilating powers of the atmosphere are not exclusively confined in their circle of operations to those febrile diseases which he has denominated contagious; they are exerted with equal energy in forming epidemic diseases which he, and indeed which are generally admitted to be non-contagious. And, therefore, we would respectfully, but emphatically inquire, how this can happen, seeing they import nothing virulent to the air. We would ask, what there is, emanating from individuals labouring under influenza, malignant pneumonia, and many other complaints,

which can so assimilate the air as to render those diseases prevalent over a wide tract of country, and yet which cannot be directly or personally communicated?

But the Professor asserts in another place \*that plague, yellow fever, and dysentery, are 'in no instances epidemic, as they have been improperly denominated by most practical writers; but that their sphere of operation is, with very few exceptions, confined within the limits to which the vitiated atmosphere extends, in which they may be engendered, or into which they may be introduced, and that in this respect they differ from ordinary epidemics, 'which appear in different and distant parts of the same place and at the same time.'

On reflection we are not surprised that he should have drawn the above distinction. A denial that plague or yellow fever is ever an epidemic, was absolutely necessary, in order to show, with any degree of plausibility, its personal contagiousness, and to explain the manner in which it swallows up other diseases. He was aware that epidemics, in the sense he uses the term, also impart their character to other disorders; and consequently it would not answer his purpose to consider them in connexion with plague, yellow fever, and dysentery, especially as they are, for the most part, incommunicable diseases.

If, however, the Professor's doctrine of febrile contagion be true, we must infer, that there are two methods by which prevailing disorders swallow up other complaints. One of these he has explained, by supposing that febrile contagion assimilates an impure atmosphere into a poison like itself; of the other he says not a word. But such an inference is so unphilosophical, nay, so contrary to the simplicity of nature, that we presume no one will have the confidence to maintain it. And again, if his doctrine be true, and supposing that there is but one method by which diseases are assimilated, and this we regard as an indubitable fact, we are

\* Vide Appendix to Thomas's Practice, p. 793. Ed. 1815.



conducted to the equally ridiculous conclusion, that those disorders which appear in different and distant parts of the same place and at the same time, "derive their epidemic character from an assimilating fermentation" of the air, similar to that which is said to occur during an epidemic yellow fever or plague.

Now we confess, that the deductions from the plainest reasoning on this subject, are to us entirely satisfactory; yellow fever prevails throughout a city, because impure air is extended over that city:—influenza, and some other disorders, prevail throughout an empire, because a morbid atmosphere is extended over that empire. These conditions of the air must exist immediately antecedent to the occurrence of diseases, because they are the causes of diseases, and consequently, if the same causes, under like circumstances, always produce the same effects, we may safely conclude, that these states of the atmosphere are as capable of producing an epidemic as a single case of disease.

But to conclude; we leave it with our readers to decide, whether the brief expose just given of the whole matter, is not more consonant with sound logic, than the extraordinary supposition, that the "divine something in the air of Hippocrates and the constitutions" of Sydenham and other writers, mean nothing more nor less than that human bodies, labouring under certain morbid derangements, have the power of exhaling certain poisons all different in their nature, and yet each capable of assimilating the atmosphere of villages, cities, and extended regions, so as to render all diseases similar in form and character. In our opinion, the great error in the Professor's doctrine, is, that it explains too much, and consequently runs into an absurdity: we therefore reject it, and shall remain steadfast in the faith of our forefathers. A logical theory should do no violence to any fact belonging to the subject it is designed to elucidate. In the hypothesis of Dr. Hosack, there is in this respect, faults so glaring and extravagant, as not only to prove the whole of it unfounded, but unworthy of its distinguished author.

We perhaps owe, an apology to our reader for appending to this article a critical analysis of opinions of Professor H. In excuse we plead that he was introduced to us by Sir Gilbert Blane, as an "extremely judicious" writer on the varieties of contagion, and we felt that in courtesy, we were bound to treat him with unreserved cordiality. And now, in taking leave of the volume of "Elements," we can assure Sir Gilbert, that in examining its merits, we have not been influenced by any unfriendly prepossessions; and in evidence of this, we are happy to remark, that, with the exception of the sixth section, we think it contains much valuable and interesting matter, and richly deserves the attention and perusal of every student. Whether it will materially add to the lustre of his fame, the popular voice must determine. In our estimation he has long been distinguished as a successful labourer in the field of medical science; and we doubt not but that posterity will duly appreciate his labours and honour his memory.

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*Facts and Observations on Liver Complaints, and Bilious Disorders in General ; and on those Derangements of that important Organ, and its immediate and intimate Connexions, which most sensibly influence the Biliary Secretion; with Practical Deductions, drawn from an ample share of Experience, and a close and constant Attention to this subject in various Climates ; connected by an appropriate and successful mode of Treatment, &c. &c. Fourth Edition, by John Faithhorn, M. D. formerly Surgeon in the Hon. East India Company's Service. 8vo. pp. 164. London, 1820.*

We are not quite certain how the work before us should be considered, whether as a popular treatise for valetudinarians, or an attempt to enlighten the professional reader. It is a sort of hybrid production, partaking of both cha-

racters ; but chiefly we think, of the former. The author, indeed, in his preface, avows one of his designs in its publication, to be “ to awaken the understanding of patients to a full comprehension of their situation, thereby to impress them with a necessity of soliciting professional assistance ;” and in another part of his work disclaims any intention of instructing such to administer to their own relief ; which explanation may perhaps account for the very scanty information he has afforded us on the treatment of those affections of the liver, it would appear to have been his principal object to elucidate ; and the kind of Delphic phraseology in which those parts are couched : doubtless believing that the professional and initiated are alone worthy of giving the interpretation ; and none more so than Dr. Faithhorn.

It has become much the fashion in our days, to write books, not so much with a view to advance our knowledge of the subjects, treated of as to inform the public where such information may be had, and to serve as circular letters. The present volume appears to us to be one of this class, and is much better calculated to direct the attention of his majesty’s subjects living in and about London, who may have suffered from the ill effects of warm climates, or full feeding, to Dr. F. as their best adviser in all such cases ; than to add any thing to the amount of what is already known on the interesting subjects of which it treats.

The author it appears has enjoyed extensive means of investigating this important class of diseases, as well in England, as the East Indies, in which latter country he has been employed on various stations in the Hon. the East India Company’s service—a country where it is well known diseases of the chylopoietic viscera, especially those of the liver, are as frequent and fatal as those of the pulmonic organ are in England and this country. From this circumstance, and the various and liberal display of his title page, we took up his book in the expectation of finding a subject that had successively exercised the talents

of Glisson, Bianchi, Morgagni, Lieutaud, Saunders, Farre, and Portal, still further improved and enriched. That it is capable of such improvement, the last of these able investigators frankly confesses, when he declares that its diseases (those of the liver) are less known, both as to their nature and treatment, than those of any other part of the system. But our expectations have been sadly disappointed; the little the work contains, is as well treated elsewhere, particularly by Saunders; and if we derived any satisfaction from the perusal, it was from the consideration that but little time had been occupied in exhausting its contents. We are nevertheless disposed to notice it as one of the books of the day, and affix to it our *perlegi*, if not our *imprimatur*. The author, after the example of Saunders, premises with some physiological doctrines of the liver, which, with one exception, are those of his prototype. He repeats the prevailing opinion, that the office of the vena portarum is to supply the fluid from which the bile is formed, without noticing any of the well-founded doubts that have been urged against this theory: but at the same time he favours the opinion of Haller and some other physiologists, that the liver performs some other important function in the animal œconomy, besides the secretion of bile. That much sound argument may be urged in favour of such an opinion, we well know; and are even willing to go still further, and maintain, with Bichat, that it is by no means ascertained from which system of vessels, the vena portarum, or hepatic artery, the bile is secreted. That the blood of the vena portarum is not indispensably requisite for the secretion of that fluid, and that that brought by the hepatic artery will suffice for that purpose, we have demonstrably proved to us by the case related in the Philosophical Transactions by Mr. Abernethy, in which he found healthy bile in the gall bladder of a child, aged about seven years, although the vena portarum terminated in the vena cava, without transmitting any blood to the liver.



Moreover, we object to resting satisfied with the doctrine of the vena portarum carrying blood to the liver merely for the secretion of bile, without some better reason for it than has yet been given; as calculated to lead to confined and imperfect views of that important organ. If, as Bichat well observes, the hepatic artery be considered too small for such a function, where the excretory duct is larger than it, so on the other hand it may be urged with equal force, that the vena portarum is as much too large; and besides that, there is another organ, viz. the kidney, whose excretory duct is larger than its artery. The great size of the liver in the foetal state, when there is little or no bile secreted, and cannot of course answer the same purpose which it does in the child's after existence; its early formation being coeval with the brain and heart, and much sooner developed than either; its important connexion in the foetal circulation—a large portion of the blood received from the mother passing through this organ before it is distributed to the different parts of the system; its great size and peculiar supply of venous blood in the adult state, with its comparative small excretory duct; its manifest influence over the mind, temperament, and system of man; and, finally, its great constancy in the different classes of animals, where other organs, commonly estimated more important, are wanting; are all powerful reasons to induce us to believe, that it serves some other purpose in the animal economy, besides merely supplying bile for the digestive process. The circumstance of all the venous blood of the abdominal viscera passing through this organ before it mingles in the general torrent of the circulation, of itself seems to indicate that it is there submitted to some change, of which physiologists are ignorant, and which well deserves their further investigation.

From our author's new physiological notion, that no bile ever passes into the stomach in the healthy and undisturbed condition of that organ, we beg leave unequivocally to dis-

sent; and if he has forgotten them, to remind him of the labours of Haller, Lieutaud, Morgagni, and Bichat, on this point. These able physiologists examined, with reference to this subject, a great number of stomachs, both in the human subject, and in brute animals, where this organ had remained unimpaired, and they invariably found them to contain a portion of this fluid; from which they concluded, that it there performs some important agency in the process of digestion. Indeed, so convinced were Sauvages and Bonnet of the correctness of this opinion, that they endeavoured to prove, that in those instances where the bile could not pass into the stomach by the ordinary route, a communication was obtained for it through the blood vessels;—a doctrine which has been in no wise satisfactorily proved.

A bare enumeration of the different diseases incident to the liver, would form a catalogue of no inconsiderable length. Besides those in which this organ is primarily and principally affected, giving a distinctive character to the malady; there are a multitude of others in which it is materially concerned, and claims the consideration of the practitioner. But it does not appear to have been our author's aim to take such an extended view of hepatic derangements. He has contented himself with surveying the subject much in the same way as his countryman Saunders did before him; except that he has laid a little more stress on those hepatic diseases which affect sympathetically, or otherwise, neighbouring organs, and are liable to be mistaken by the practitioner for primitive affections; whilst Saunders, on the other hand, viewed many of these affections, particularly those of the stomach and intestines, as primitively lodged there, and the hepatic system affected only secondarily. Saunders has supported his opinion by no facts, and little reasoning; and Dr. Faithhorn's descriptions are much too loose and general, especially in his large collection of cases appended to the volume, the most of

which gave us no idea of his patient's malady ; to establish any satisfactory or conclusive doctrine. The reader, we think, will find this subject treated much more ably by Mr. Abernethy, in his work on Disorders of the Digestive Organs, than by either of these gentlemen. As for those cases of asthma, cough, phthisis pulmonalis, &c. which Dr. F. attributes to hepatic disease, they are neither new in the records of medicine, nor so frequent, we think, as he would have us believe. They have been observed from the earliest times, especially pulmonic disease from hepatic derangement. Valsalva particularly notices such occurrences; and Portal, who gives similar cases from other authors, and his own practice, also records, in his memoir on the situation of the liver, (see *Memoir sur plus. maladies*,) an interesting case, in which a disease of the lungs was mistaken for an hepatic affection : a mistake quite as easy to be made, and much more likely to happen, from the greater frequency of pulmonic disease. The author makes no mention of other diseases of the thorax, which have been considered as sometimes arising from disordered liver, viz. carditis, hydrops pericardii, hydrothorax, and angina pectoris, which last, Portal has shown frequently to arise from this cause. That torpor and inaction of the intestines will sometimes arise from a derangement of the hepatic function, and that they will be rendered still more obstinate by accumulation and retention of mucous matters, which are continually secreted and poured into these organs, is very evident, and are usually noted as symptoms of chronic hepatitis ; but, at the same time, we would remark, that a contrary state, we mean diarrhœa or dysentery, is much more frequently the consequence of such derangement : for the liver can seldom be so far altered as not to afford some secretion to flow into the intestinal canal ; which secretion, being very unlike bile in the healthy state, will serve imperfectly, or perhaps not at all, the purposes for which it was intended, and will most frequently



irritate and incommode, and hence give rise to dysenteries, diarrhœas, colic, cholera morbus, melæna, &c., each of which are frequent occurrences in chronic hepatitis; but more particularly the two first.

Our author has given us nothing new in his view of acute and chronic hepatitis, if we except his assertion, that the latter disease is as frequent in England as the former is known to be in the East Indies, where one-third of the complaints are said to be derangements of the liver; an opinion not warranted, either by our author's proneness to see his favourite disease, where others would not suspect it, or the most casual examination of their bills of mortality. Indeed, if he were to set down all the diseases of the abdominal viscera as lesions of the liver, how could he for a moment imagine, that they would equal those of the pulmonary system; the bane, we had almost said, the endemic of England? But although he has overrated their frequency, we have no doubt that they are much more prevalent both in England and in this country, than is commonly allowed. Every author who has treated of them bears testimony to this effect, which should at least inspire us with a renewed spirit and eagerness to investigate, and obtain a better knowledge of them.

In his directions for the treatment of the different affections he has noticed, there is little to enlighten the valetudinarian, or the professional reader. Here, as elsewhere, he has trodden in the footsteps of Saunders; and though we do not know that he could have followed a better guide, yet when an author attempts to write a book, it is natural for the reader to look for something more than mere compilation.

On the whole, therefore, we might well have been spared the present volume seeing that it contains nothing new that is of any value, and consequently does not remove the desideratum of a work, that shall treat fully of the various derangements of the hepatic organ. From such a labour



we confidently anticipate an important accession to our knowledge of the fevers of hot climates, and even of our autumnal intermittents and remittents: for there is reason to believe that these fevers are more connected with hepatic disease, and possess more symptoms in common with acute and chronic hepatitis, cholera, &c. than has heretofore been suspected. Not to rest our opinion on the yellowness of the skin, and the bilious vomiting, the frequent attendants of these diseases; we would call the attention of our readers to the extraordinary coincidence that these affections exhibit in the condition of the urinary secretion. In the milder forms of these fevers, we find this fluid lateritious, flame-coloured, or bilious, and in small quantity; in the more aggravated and malignant forms, it is either highly charged with the same materials, and alternating with dysury, or else turbid, bloody, and depositing a sooty sediment. The same conditions are frequently observed in aggravated cases of hepatitis, especially in tropical latitudes; and also in cholera, melæna, and the more violent forms of bilious colic. Now if the observation of Senac be well founded, that lateritious urine occurs only in fevers of intermitting and remitting type, or rather in fevers arising from marsh miasmata, which we presume would be the better restriction; then will these analogies go far to prove the approximation of these fevers to other affections of the bilious system, as well as the identity of their causes; and would be an additional motive for maintaining that the yellow fever is not a specific disease, propagated by a specific contagion. The great tendency to hemorrhages from the nose, stomach, and intestines in the diseases we are attempting to place in parallel, has never sufficiently attracted the attention of physicians, as mutually casting a light on their nature and rationale; although their occurrence in each of these diseases has been often noticed. Galen considered epistaxis from the right nostril as a diagnostic mark of hepatitis and melæna; and hemorrhoids were supposed to be

most commonly caused by engorgement of the liver. The same disposition to hemorrhage from the mouth, nose, and intestinal canal, has been remarked in bilious remittents and yellow fever, by numerous writers. Besides these, there are other coincidences, chiefly in symptoms, that accompany these complaints, which are remarked to occur with peculiar aggravation and intensity, seeming to depend on some peculiarity of the morbid action, rather than on those circumstances that give rise to them in other diseases; we mean the unquenchable and tormenting thirst, the disordered and irregular state of the intestinal canal; and the mortal restlessness and agony frequently attendant, when the other symptoms do not indicate any particular danger.

Nor are the above diseases the only ones in which there is reason to believe the hepatic function is materially concerned, and which hitherto has been almost entirely overlooked in treating of them. Repeated autopsic examinations have convinced Portal, that many eruptive diseases, especially the herpetic, arise from hepatic derangement, and are only to be successfully treated by restoring the functions of the liver to their healthy condition. The wonderful influence the liver exerts over the mind and moral condition of man, has been observed from the earliest times; but it has not, it appears to us, attracted sufficient attention, as the cause of mental alienation. And, finally, almost every author who has paid particular attention to diseases of the liver, has become impressed with the belief that they are much more frequent than is commonly supposed, especially as complicated with other affections, by which they are disguised, and pass unsuspected. Boerhaave believed this organ to be in fault in almost every chronic affection, and the opinion of many others of great authority in medicine might be quoted to the same effect; but we have said enough to show, that the influence of hepatic derangements in the production of other diseases, is still imperfectly understood; and we sincerely hope, a subject so fraught

with interest, and rich in expectancy, will, ere long, find an abler investigator than the author of the book before us.

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- I. *Report of the Committee of the Medical Society of the City and County of New-York, on the Causes and Character of the Epidemic Fever which prevailed in Bancker-Street, and its vicinity, in the Summer and Autumn of 1820. Published by order of the Society. New-York. pp. 38.*
- II. *A Statement of Facts relative to the late Fever which appeared in Bancker-Street, and its vicinity. Published by order of the Board of Health, &c. New-York. pp. 28.*
- III. *An Account of the Yellow or Malignant Fever of Philadelphia, in 1820, by Samuel Jackson, M.D. President of the Board of Health.*
- IV. *Report of the Joint Committee of Councils, relating to the Malignant or Pestilential Disease of 1820 in Philadelphia. pp. 55.*
- V. *Report to the City Council of Savannah, on the Epidemic Disease of 1820, by Dr. Wm. R. Waring. Published by order of Council, Savannah. pp. 78.*
- VI. *Report of the Committee of the Physico-Medical Society of New-Orleans, on the Epidemic of 1820. New-Orleans. Published by Order of the Society.*

WE have thought it our duty to sum up, for our distant readers, the great mass of evidence here adduced on a subject most intimately connected with the welfare of populous cities. Our zeal for promoting inquiries into the real causes of our summer epidemics, is still further stimulated by the desire we feel to diminish the distressing



shackles and unnecessary impositions of quarantine, to which the American commerce is subjected in European ports, in almost every season of the year. The French government, with a wisdom and spirit worthy to be imitated by other nations, have lately instituted an inquiry into the nature and origin of tropical fevers, in order to test by experiment and the suffrage of physicians practising in countries where these fevers prevail, the question of their contagious attribute; and from the results of such investigation to found a system of Health laws, which shall meet the actual state of things, without subjecting them to the necessity of guarding against imaginary dangers. And we confidently expect that results, thus legitimately deduced, will enlighten European governments on the true character of a disease, which they have, for the most part, viewed at the dim distance of three thousand miles; and which (it would seem still further to increase the terror and dismay attendant on uncertain and exaggerated reports) has been denominated the American plague. We regret to say, that some few are to be found, even in this country, who have preferred to minister to these fears, rather than put themselves to the trouble of a severe examination of facts, which would dissipate the delusion.

The above collection of papers are recommended to the serious perusal of the reader; for with one exception, they are divested of party feelings and prejudices; nor has the least breath of professional hostility endangered the truth of the facts they contain.

We are indeed arrived at a period of our controversies, in which much information is diffused, a greater range of observation has been obtained, and an analytical philosophy has been substituted in medicine, for problematical theories; the oftener, therefore, impartial reports on epidemics are submitted to the severe scrutiny of public opinion, the sooner must reason and the love of the public weal point out the effectual means to arrest their destructive



course. It is to that sound prevailing spirit, that the inhabitants of many of our large cities are indebted for the success with which the progress of yellow fever has been met and arrested in many instances of late years, soon after it had broken out with the most threatening aspect, and long before the change of season alone could warrant the expectation of such a result.

I. The first report was issued from the Medical Society of the City and County of New-York, after it had been ascertained that no information on the subject of a mortal disease, which ravaged a populous district during the summer and autumn, had been, or would be, furnished to the public, by the Board of Health. It is, indeed, in the recollection of every inhabitant, that during the period of its existence, no report was as usual published by municipal authority, that could give the least alarm; and to this day, no documentary evidence is to be found on this interesting subject, except that a greater mortality by 329 deaths than in the former season (when the yellow fever was declared to exist in Old Slip) occurred, during four months, from a *serious fever* in Bancker St. (vide City Inspector's Report, for 1820.)

The Medical Report offers two distinct subjects of inquiry; first, the nature and character of Bancker Street fever: secondly, the causes and their correction, especially in relation to the organization of the Board of Health. It is established in this Report:

1st. That the district of Bancker Street and its vicinity was, during the warm season, in a most deplorable state of filthiness, and that its inhabitants were in so wretched a condition, as to have attracted the attention of the Grand Jury, who most emphatically represented the neighbourhood to the Court of Sessions, as affording all possible materials calculated to kindle and keep alive pestilence.

2dly. That the large population in the Eastern part of that Street, of 1566, composed of blacks and whites, were

literally crowded together in various infected parts of the district ; some houses and lots of which are designated as containing unusual numbers ; as, for instance, 144 on two adjacent lots, and 81 on another.

3dly. That great quantities of fermenting and putrid materials were accumulated in the cellars and yards of many old decayed and ill ventilated buildings.

4thly. That the distemper was of short duration, of a remittent type, generally neither marked by torpor of the sensorium, nor by delirium of mental perceptions; marking great anxiety in epigastrio, excessive bilious vomitings, jaundice, and, in some instances, black vomit. That autopsic examinations uniformly confirmed a malignant determination to the stomach, liver, gall-bladder, and spleen. That this fever bore no resemblance to any kind of typhus in its symptoms, nor by its circumstances of duration or of season; and that it was similar to the yellow fever.

5thly. That one-third of the inhabitants of Bancker Street experienced the epidemic in various degrees ; the proportion of blacks to whites affected was as three to one. That 150 deaths are supposed to have taken place in this street, the hospitals, and other situations, to which they were removed. And that authentic instances of this fever contracted in Bancker Street, had been ascertained to have existed at Powles Hook, and at Jamaica, L. I.

The report contains also several other interesting matters, which our present limits will not permit us to notice.

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II. The second paper is an attempt to invalidate the facts contained in the preceding Report of the Medical Society, and contains a positive asseveration, that the disease in question was *not* yellow fever. It purports to have been drawn up by the medical members of the Board of Health, and we regret to say, has been laid before the public under the auspices of that honourable body for it is pervaded by a spirit unworthy the sanction of constituted authorities, whose duties we think can never

call them to mingle in controversies purely professional, especially after the circumstances that had given rise to them had gone by. But, that the gentlemen who at that time composed the medical commission, should have been averse from admitting that a fever which originated and was confined to a small district of the city at a considerable distance from the water side, and of course could not be referred to a foreign source, and did not moreover extend its influence beyond the foul emanations of the place, was malignant or yellow fever, did not surprise us ; knowing, as we do, that these gentlemen are the avowed advocates for the contagious character of that fever, and its exclusively foreign origin. We have accordingly regarded their present performance more in the light of an effort to guard their individual character from being merged in the prevailing doctrines of the country, than as a candid exposition of the actual occurrences of the endemic prevalence in question. On this account, we shall decline giving our readers an analysis of its contents. Its perusal forcibly recalled to our minds the following anecdote related of Swift, which will give our readers a tolerable idea of what the statement was intended to effect, without putting them to the trouble of reading it. One day, much to the annoyance of the worthy dean, an immense multitude collected before the deanery, to witness an eclipse : on which he sent forth his beadle, decorated with the pomp and circumstance of office, to proclaim that it was his good will and pleasure to postpone the said eclipse to next day. Many acquiesced, but some denied the dean's right, as well as power, to control the laws of nature.

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III. We can assert with confidence of this masterly Report, that it is drawn up by a physician, whose perfect acquaintance with the subject is divested of party prejudice. We do not know that he has ever enlist-

ed on either side of the controversy, or engaged his professional reputation for importation or non-importation. His sole object i impartially to discharge the responsible duties of a public medical officer. The report is therefore as candid as instructive.

That the prevalence of malignant diseases is always under a particular agency of atmospheric constitution, is proved by several successive periods of time having elapsed, during our unrestrained intercourse with the tropical colonies, (the ordinary abode of malignant fevers,) in which neither our shipping nor our ports were infected. From the year 1762 to 1793, the yellow fever had not appeared as an epidemic in any part of this country. The period of the revolutionary war of America did not furnish to our physicians a single instance of this disease, though large bodies of troops were then incessantly transported to the Antilles, and thence landed in our country. There was also a period of general health in all our interior and maritime situations, from 1805, until a novel and fatally malignant form of disease commenced eastward, and gradually spread gloom and dismay throughout the United States. The yellow fever was then, and for many succeeding years, almost a stranger among us, until 1818. The history of these vicissitudes, connected with pestilence, is therefore very important, and is satisfactorily treated by the author before us. We have also in this paper, a candid account of a pestilence, which carried off eighty-three victims only, in a population of 120,000, at ten different parts, of a large city, some at a great distance from each other; the disease bore a threatening aspect in the intensity of its symptoms, and was successfully checked before cold weather. The wise measures adopted by public authority, on the principle of local sources of infection, proved to be sufficient, without recurring to a calamitous dispersion of the inhabitants.



We learn, by this report, a still more important point of medical doctrine, and of extensive application in practice, which seems to have been much neglected by our modern nosologists, viz. the existence in yellow fever, of different degrees of the disease, according to the greater or less intensity of the infecting causes.

The *first class*, which is minutely described in all the variety of its Protean forms, transcends, in its rapid succession of deadly symptoms, the resources of the most skilful practitioners, with the power of the best remedies. The efficacy of these may again be depended upon in the *second class*, which is not less malignant in its nature, nor very dissimilar in its general form from the worst kind. The *third class* is easily recognised, because it possesses the same features and outline of symptoms; but it yields with great facility to the remedies employed. Sometimes it is ephemeral, and providentially makes up the greater proportion of cases, as we have seen in no less than seven epidemics; although we believe, with the writer, that the first class predominated at Hodge's wharf during the last season. We have seen it generally so at the beginning of the epidemic, and during the last warm days of the autumn. The inferences drawn from this interesting view of the subject, appear to us so important, that we must be permitted to transcribe them in the words of the writer.

"Contagious diseases, when epidemic, or otherwise, will differ in force, as it respects individuals, according to the state of the system or constitution of each, but never present a constant and permanent difference—being, according to the situation, mild and simple in one place, and malignant and aggravated in another. Does not this fact render the supposition highly probable, that the poison which produces yellow or malignant fever, is evolved in the atmosphere, or collects in certain spots in greater or

less quantity, or from some causes becomes more or less highly concentrated in them, and thus occasions different degrees of poisoning of the system? We observe similar effects to arise from the exhibition of poisons in different quantities. Thus a certain number of individuals may take a certain quantity of arsenic; they may all be more or less affected, but will all recover without difficulty; others may take a larger dose, which will produce a greater degree of disorder, and a more violent train of symptoms; but by timely and prompt applications, the larger proportion will recover: others, again, may receive a still stronger dose, which will occasion such organic lesions, that nearly all will fall victims to its deleterious operation, one or two only escaping, as it were by miracle. These effects bear a strong similarity to the different classes of yellow or malignant fever, the poison producing which, "like arsenic, acts chiefly on the stomach, and produces a lesion of that viscus, in proportion to the quantity introduced into the system."

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IV. Great honour is attached to that transcendant philosophy, which, on controversial subjects, can prejudge and adopt an opinion of the *true* side of a question, cordially support and vigorously contend for it against a host of antagonists; although, in the defined limits of human life, perhaps, one may not see the general adoption and triumph of the question, which had created many enemies to him during the period of its most vigorous contest. But we must confess, there is also much wisdom in that prudent and considerate judgment, in difficult and abstract questions, which commands opinion itself to submit to the experience or authority of a majority of competent judges. Among physicians who have taken part against the belief of the domestic origin of the yellow fever, there are many, no doubt, who would prefer to be guided by a majority, who like themselves, are conversant with the disputed subject. or who feel equally interested in it. To these

wise men, the Report of the joint Councils of Philadelphia is particularly recommended, in order that they may be benefited by the unanimous decision of respectable scientific institutions, which were formerly arrayed against each other, but have at last acknowledged and proclaimed, "*that a city may become unhealthy, even to the malign state, from offensive matters brought in vessels from other ports, and that a deleterious atmosphere may arise among us, from accumulated filth about the wharves and streets, from the want of cleanliness in families, and from inadequate ventilation.*"

To Philadelphia we must therefore look for good examples in our medical controversies. To her physicians we must refer those modern partisans, who have not only presumed to decide the question in their own way, but have branded their antagonists as disgraceful members of their profession !

This report contains the official answers to the interrogatories of the joint Committee of the Councils ; from the College of Physicians ; from the Academy of Medicine ; from the port and Lazaretto physicians ; from several respectable individuals, whose particular or professional inquiries have been directed to the means for correcting all sources that might contaminate the air of the city during warm weather ; as also copious extracts from the pamphlet of Dr. Jackson, which we have already mentioned. Those authorities, acting in the greatest unanimity, have it in contemplation, to remove all the buildings from the east side of Front Street, according to the original plan of William Penn, the wise and intelligent founder of Philadelphia.

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V. The modest author of this important document, does not present himself with imposing academic titles ; but he notwithstanding, appears well qualified for the task intrusted to him by the City Council of Savannah. We have not in recollection a more satisfactory etiology of the causes and nature of the summer epidemics of the southern

states; and although we think we have acquired some experience for ourselves, we confess, we did not know that so much could be said in support of the unity of miasmatic diseases. It is true, we had never seen them so distinctly and simultaneously existing in the same population, altered in their forms only, by the degrees of intensity of the deleterious causes, succeeding each other in different parts of the same city; from the *simple remittent fever*, to the bilious, the prostrating remittent, the continued type, and black vomit. We have here the evidence, that all of them have been seen at the same time, in the same house, and among the members of the same family. Neither can we object to the appellation, given to the last, of continued type; because the author confines it to that rapid and unexceptionably fatal form of fever, which is resolved by death within the short space of a few days, rarely showing any distinct remission of its febrile, or other fatal symptoms. This fourth species of yellow fever, which Dr. Jackson of Philadelphia has called the first class, is, therefore, the same which we have often witnessed, existing in a certain proportion of individuals, among a greater number of *remittents*, or *bilious* and *prostrating remittents*. Admitting these facts, which we have not the least reason to suppose are misrepresented, or obtained from a limited range of observation, we can account for the simultaneousness of various diseases, frequently noticed in the histories of memorable destructive pestilences, of the plague especially, which has been observed in so many different shapes. In that of Marseilles, we are told there perished 15000 persons with the symptoms of jaundice and black vomit. In reading over ancient records respecting the croup of children, (*angina trachealis*,) we find it was first observed during an epidemic of malignant catarrh and putrid sore throat; and we also experienced, six or seven years ago, throughout the United States, the prevalence of a mortal disease, which varied in form and name. The fact, here stated could not, therefore be reconciled with



the doctrine of imported febrile contagion, being continued and propagated by a primordial material, nor even with that which limits its communication to the additional power of an impure atmosphere. The author before us is not at a loss for pressing objections against both. He produces evidence ; 1st. That yellow fever has been yearly observed in Savannah during 14 years, without being propagated by contagion, though that city is always immersed in a humid and unwholesome atmosphere. 2dly. That no individual, seized with it, and removed to the rice plantations, so justly reputed unwholesome abodes, had ever communicated it to any other person. 3dly. The disease variously progressing, abating in some parts of the city, while it was aggravated in others, and never observed to be communicable to the nearest or most habitual attendant. 4thly. That it could not be traced to foreign ships, because the two fatal cases had taken place, the one on the 7th, the other on the 10th of May; before the first arrival in the port of a vessel from the West Indies on the 22d of that month. That on the same day a government vessel, commanded by Capt. Newell, left the island of Ossabaw for New-York, having lost one of her crew by the yellow fever; another died during the voyage, and a third in the marine hospital, of that port declared by the Health Officer to be a most malignant case ; whilst none of the crew had communicated with the main land or any of the shipping. 5thly. That another vessel, from the coast of Africa, arrived only on the 22d of July, after 50 deaths by the fever had occurred in Savannah. All these facts, and many others, must go far to destroy the belief of importation and contagion, even if it be modified by the additional help of impure air, in our country, and thereby made "to keep good company". (Vide page 30.)

We rejoice to find in this report many novel views on the pathology of yellow fever, which may probably lead to inferences highly useful in its treatment.

Dr. Waring has ascertained, by the examination of 60 dead bodies; 1st. That the inner or mucous membrane of the alimentary canal, and principally of the stomach, is, by irritation or inflammation, the seat of the disease. From the morbid excitement in this central point, sympathies are created to operate upon or affect all the other functions of the human system; on the verge of dissolution patients have been conscious of it, as if a deadly poison had been there concentrated.

2dly. That the black matter is neither blood nor bile, but a material deposited in the stomach from the small vessels of its inner coat, which in the natural state contain serum, but in this disease are distended with red blood, which undergoes a change, or a transformation into black matter. This preternatural secretion is sometimes preceded by the rupture or ulceration of the same vessels, and then hemorrhage ensues *before* it is formed. Sometimes this black matter has been effused without much alteration of the coats of the vessels, and even apparently relieving the stomach and leaving on it no mark of inflammation; but at other times a sphacelated ulceration, ensues, and then hemorrhage occurs *after* black vomit. The same phenomenon may take place, although rarely, in any part of the intestinal membrane, and even on the tongue. We feel the more satisfied that this view of the subject is most probably true, since the author's observations were conducted with minute attention to the hepatic and arterial systems, after having well ascertained, in each subject, the previous symptoms or effects of the disease. His conclusions are furthermore confirmatory of, and in conformity to, those which have been heretofore published by such accurate observers as Rush, Physick, Irvine, Jackson, and others. We regret that we have neither time nor room to dwell on this abridged account of the performance in its application to the different classes or degrees of the disease; its occasional alteration from one form into another under certain circumstances; its

operation, assimilated to that of certain poisons, arsenic for instance, which is extended by sympathy from the organs of digestion to all the functions of the system, and even developed on the surface of the skin ; and the indications of treatment which have been attempted and justified by some success, and may probably be farther improved.

The whole of this valuable Report recommends itself to the attention of medical men. But we would beg leave, for the benefit of his readers, to solicit the author's attention to a more methodical arrangement of his writings, by sections, chapters, or numbered paragraphs.

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VI. We are presented here with an abridged view of the local, natural, and artificial evils existing in the city of New-Orleans. The yellow fever is called an *endemic*.

The remarkable fact is stated, that, as in former years, the earliest as well as the latest instances of the distemper were observed on board the steamboats and shipping ; and the same are accounted for by their polluted condition, and their proximity to places where the greatest accumulations of the filth of the city are found. Notwithstanding which, the reporters declare, that they are not able to dwell on the probable causes of the fever, from a deficiency of data, and other general difficulties attending such inquiries. By a resolution also of the Society, the committee were directed to avoid the controversy on contagion. It is remarked, that respecting the best modes of treatment, and the various remedies which are severally noticed, an humiliating inference is presented that the yellow fever must be still considered an opprobrium to medical science. We therefore must say, that very little else was in contemplation by this report, than an official act of the institution (which we believe has been recently established) to testify their

zeal and attention to professional duty, and the task of promoting the different branches of the healing art, and with it, the public welfare. The report contains a short but accurate description of the yellow fever; bills of mortality for the last four years, amounting to 5874; meteorological tables, and a list of the present members of the Physico-Medical Society.

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### COLLECTANEA CLINICA.

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*A singular case of Disease arising from an Injury received in the Lumbar Region, by Dr. Vine Utley, of Lyme, Conn.*

On the 24th of August, 1820, I was called to visit Capt. D. W. of Lyme, aged about 60 years, who was labouring under some affection of his kidneys, an enormous tumefaction of the left inferior extremity, and the scrotum. He had received an injury in the lumbar region, rather on the left side, and a serious contusion of the thighs, about nine months before, by a fall from his horse. Four or five weeks after this accident, he voided bloody urine in considerable quantity, which has continued more or less ever since; and about two months after this, the left inferior extremity, and the scrotum began to swell and become painful. For these complaints he was bled, had different applications to the limb, and took various medicines, by the advice of the physician who attended him. I found him confined to his bed, with the limb enormously swelled quite to the hip; the skin tense and shining; and the scrotum about the size of an ordinary hydrocele, very tense, and indicating an effusion in the tunica vaginalis. He complained of severe pain in the region of the kidneys, and throughout the swelled limb; his urine was small in quantity and now and then tinged with blood. Pulse more frequent than natural. I



directed him to take of calomel gr. iij. night and morning, and of tincture of opium gutt. xv. daily. Several birds-eye punctures were made in different parts of the limb, from which oozed a lymph-like fluid for several days, without abating the swelling. This course was pursued for six days without any apparent effect, when I added the hot bath, which was employed twice at short intervals, and with the best effects; the patient soon after having a copious flow of urine, discharging in the course of one night full two gallons; and in about 24 hours after the swelling of the extremity and scrotum had entirely subsided. At this time also, a full ptyalism supervened, which was however subdued in a few days by the ordinary means, and the patient felt himself in tolerable health, without any swelling of the extremity or scrotum. About a month after, he was again suddenly seized, much in the same way as before with pain in the region of the kidneys, voiding bloody urine, and the limb and scrotum swelled to their former size with excessive pain. I again had recourse to the plan of treatment that had been before so successful, with precisely the same results in every particular; and the patient, as before, after suffering a ptyalism, so far recovered as to be enabled, to return to his accustomed avocations, complaining only of some debility, and voiding occasionally bloody urine. He continued in this state four or five months, when he was again attacked as before, except that now it was much more violent, and the pain in the lumbar region so excruciating as to give him rest neither night nor day. The urine was now very turbid, bloody, and mixed with coagula: the swelling occurred in the left extremity and scrotum as before, and soon pervaded the right extremity also, which became nearly as large as the other. He now suffered extremely from anorexia and vomiting, and the alvine evacuations were only obtained by purgatives. The symptoms were of so serious a character as to preclude the hope of relief, and nothing more was attempt-

ed than to palliate the distress ; and the patient died on the fourteenth day of the third attack. I could not obtain permission to make any examination after death.

### REMARKS.

We regret exceedingly that Dr. Utley has not furnished us with a more minute account of the above case ; and that he was prevented from making the autopsic examination. As it is, however, the case is highly interesting, and worthy of being recorded. There can be no doubt that the primitive lesion was in the kidneys, and the effusion into the scrotum and the inferior extremities we should explain, by considering it an effort of the *vis medicatrix naturæ* to establish a vicarious discharge, from the inability of the system to free itself of its excrementitious fluids through the ordinary channel. Hence it was that the effusion was suddenly taken up, by producing a profuse diuresis, and recurred again and again after some time had elapsed, and the kidneys had returned to their state of inaction and torpor. Hence also, it was, that the tumefied parts were distressingly painful, showing that it was not a simple anasarca.

We would also suggest the probability, that a stricter scrutiny into this case would have presented points of analogy between it and phlegmasia dolens. The tumefaction being confined to one extremity until near the termination of the case ; the excessive pain in the tumid parts ; the lymph-like deposit ; and its arising from injury of a neighbouring organ, appear to us to give room for the suspicion. It is probable that a more correct knowledge of the case, in all its bearings, might have enlightened us on the nature of a disease which is as yet but little understood.

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## INTELLIGENCE.

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### *Flatted Skulls.*

One of the most curious problems in craniology, is that which relates to the configuration impressed upon the bones of the head by art.

It has been long understood, that the human head was capable, by compression, of assuming almost any form, between a trencher and sugar-loaf.

The skulls received by Dr. Mitchill from Col. T. H. Perkins, are of a remarkable character, and were brought from the Columbia River, on the N. W. coast of America. They are compressed as if by the action of a heavy weight, or strong mechanical power, acting downward and backward from the frontal bone. This pressure must have been begun in early infancy, and continued for the whole time, during which the bones were yielding and flexible. The consequence is, that the os frontis is depressed almost to an angle of eight or ten degrees. The parietal bones are crushed down in a similar manner; and brought as nearly as possible to a level surface. In two of the skulls, the coronal and sagittal sutures are much less distinct than common, and in some parts, almost obliterated.

The most evident effect of this violence to the cranium, is the protrusion of the parietal bones, behind the ears, to make room for the squeezed and displaced brain. This is such as to impart to the head a deformed, monstrous, or rickety appearance.

It is understood that the mechanical power thus applied to the head, widens or enlarges its base, as well as its summit. The individuals who undergo the operation, are the

children of the more considerable and important families. The lower and plebeian orders are not honoured with the distinction of *flat heads*. Such a scull of the rounded form, which nature gave it, accompanies the others, and makes a striking contrast. Living witnesses attest, that the men whose heads have been severely pressed and tortured, possess a full proportion of mental and corporeal vigour.

The case is very curious in a physiological and moral, as well as in an anatomical point of view.

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*Petrified Elephant's Tooth.*

The petrified tooth from Tuscany, forwarded to Dr. Mitchill by Dr. Albers of Bremen, is justly conjectured by the savans of Europe, to have belonged to one of the elephants that accompanied the army of Hannibal the Carthaginian commander, when he invaded Italy. History informs us, that he transported these large animals over the Pyrenees, the Alps, and the Apennines; and across the Iberus, the Rhone, and the Po. His line of march was sufficiently near to Florence, to countenance the present supposition. And the belief is strengthened by the fact, that the processes or eminences on the crown of the tooth, are those of the African, and not those of the Asiatic elephant; which marks are perfectly distinguishable.

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*Structure and Functions of the Batracian Reptiles.*

There are two animals reported to possess a double set of respirarory organs, gills and lungs. These are the siren of Carolina, and the proteus of Carniola. This extraordi-



nary organization, is so generally believed, that the most correct and modern zoologists describe it as well ascertained.

Latterly, however, the decision of M. Schriebers at Vienna, and M. Cuvier at Paris, on this point have been called in question, by Professors Rusconi and Configliacchi, at Pavia, who deny that any creature exists, who, in a complete and perfect state, has such a twofold organization for breathing. They have satisfied themselves by dissection, that in the proteus, the viscus, believed to be lungs, answers a very different purpose, and more resembles the air or swimming bladder of fishes than anything else; and the former of these gentlemen has persuaded himself, that the *siren* is not a perfect animal, but only the larva, or tadpole of a water salamander. The two elegant quarto volumes, with finished plates, have not exhausted the subject. The letter of the late Dr. B.S. Barton to M. Schneider of Saxony, has been very much in request, for shedding light upon the inquiry. And indeed, this gentleman has described a pair of organs in the American *siren*, so very much like those in the European proteus, that they may be conceived to be similar both in constitution and use. For the purpose of settling the controversy, Dr. Mitchill has taken measures for procuring the *siren*, and examining anew its structure. In the mean time, we understand he has forwarded to Pavia a copious extract of Dr. Barton's Memoir, with an opinion of his own, that as the airsacs of the proteus are not lungs, so the organs of the *siren*, that have been mistaken for lungs, are, in reality, airsacs of another kind and function. Consequently the *siren*, like the proteus, is a perfect animal, breathing by means of gills, which endure through the whole of its life.

*Curious Construction of the Vertebral Column in a Species of Sturgeon.*

It has been long known, that in many species of fish, the skeleton is composed of a substance that partakes more of the nature of cartilage than bone. They have been hence denominated cartilaginous fishes. Some of them, nevertheless, as the lamprey and a few species of ray, have the vertebræ of the back composed of a material that is neither bony nor cartilaginous, but which seems to be a composition of membrane and gelatine. A species of acipenser or sturgeon has been discovered on the coast of New-York, whose vertebral column exhibits the membrano-gelatinous structure, except the lateral and spinous processes, all of which are tipped with bone. This curious organization is accompanied with another peculiarity; the vertebræ, though neither bony nor cartilaginous, show the disposition to separate into joints.

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*Campanula Graminifolia.*

WE are told in one of the London Medical and Physical Journals, that Dr. Salvatori of Petersburg has employed the leaves and flowers of the *Campanula, Graminifolia* in epilepsy with the most fortunate results. The following is the formula employed.

R Herb. et flor. Campanul. graminifol. unc. semis: infuse per horam in aquæ fervidæ lib. una: deinde coletur; et sumat vasculum ter de die, scilicet mane, ante prandium, et vespere ante somnum.

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*Succedaneum for Peruvian Bark.*

FROM some observations by Dr. De Ruhl, in the October Number of the Journal Universel des Sciences Médicales, it appears that the *Lepidium Ruderale* is an excellent remedy in intermitting fevers. He gave it after the following manner: half an ounce to an ounce of the herb infused for

fifteen minutes in a pint of boiling water, and strained ; the patient to take one ounce of the infusion every two hours during the apyrexia.

*Antidote for Arsenic.*

DR. Chisholm, in a paper read to the Society in Geneva, states that the juice of the sugar-cane is the best antidote known for arsenic. It has been tried upon various animals in the West Indies with uniform success. Its power in the Island of Nevis is generally known.

*Lon. Med. and Phys. Journ.*

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*Remedy for Psora.*

DR. Edmund Porter of Hunterdon Co. N. J. writes to us, that he has obtained great success in the treatment of Psora, by the internal exhibition of the Spirits of Turpentine, even after all the ordinary remedies for the disease had failed. He used it in doses of from five to thirty drops daily, until the disease entirely disappeared, which was usually in the course of fourteen days.

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*Medical Properties of the Potatoe Plant (Solanum Tuberosa.).*

JUDGING from the general character of the genus solanum, Dr. Latham thought it probable that the species tuberosa might also possess medical properties, and he accordingly submitted it to the test of experience. An interesting account of his experiments with the extract, are given in the 6th vol. of the Transactions of the College of Physicians, London ; from which it appears, that as a narcotic, it is to be preferred to Hyoscyamus, or Conium, and that it may be employed in many affections where the above plants have been recommended. From seven pounds of the leaves and stalks, he obtained nearly a pound of extract, which he exhibited in doses of from half a grain to three grains, thrice a day, and even in some instances to the extent of six grains.

*Vestiges of a FÆTUS in the Human Body.*

M. Olivry of Quimper, in the *Journal Universel* of September last, relates the case of a young lady, aged 17 years, who had been under his care for some time for different lymphatic diseases. Her catamenia appeared at the age of fourteen, which seemed in some degree to improve her general health; but soon after her eyeballs, which had been slowly increasing in size for some time, became so large as to prevent the eyelids from covering them, and indicated a hydrophthmia. Her lymphatic disease finally proved fatal, and on dissection, the last dorsal vertebra, the two first lumbar, and the sternal extremity of the last right rib were found carious, with steatomatous tumours in the mesentery. The largest of these tumours appeared to be covered with fibrous membranes. It was opened, and the soft parts detached by submitting it to the operation of boiling water, when it was discovered to contain the petrous portion of a temporal bone of a subject about the age of the girl, with a canine tooth inserted into the mastoid process.

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*Trustees of the College of Physicians and Surgeons.*

THE Honourable the Regents of the University of the State of New-York, have appointed Samuel Moore, M. D. and James R. Manley, M. D. Trustees of the College of Physicians and Surgeons of this City, in the place of Drs. W. Moore and R. S. Kissam, resigned.

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*State Medical Society.*

At the Anniversary Meeting of the Medical Society of the State of New-York, held at the Capitol in Albany, February 7th, 1821, the following gentlemen were elected officers of the Society for the ensuing year.

SAMUEL L. MICHILL, of New-York, President.

PETER C. ADAMS, of Greene, Vice-President.



CHARLES D. TOWNSEND, of Albany, Secretary.

JOHN DOWNS, of Rensselaer, Treasurer.

CENSORS.

FELIX PASCALIS,	}	<i>Southern District.</i>
JAMES R. MANLEY,		
CHARLES DRAKE,		
WILLIAM BAY,		
T. ROMEYN BECK,	}	<i>Middle District.</i>
PETER WENDELL,		
AMOS G. HULL,		
JOHN H. STEEL,		
ABRAHAM ALLEN,	}	<i>Eastern District.</i>
AMOS G. HULL,		
HEZEKIAH L. GRANGER,		
LAURENCE HULL,		
HENRY MITCHELL,	}	<i>Western District.</i>
ABRAHAM ALLEN,		
FELIX PASCALIS,		
WESTEL WILLOUGHBY,		
T. ROMEYN BECK,	}	<i>Committee of Correspondence.</i>
ERASTUS D. TUTTLE,		
JAMES DOWNS,		

Doctors Peter Wendell and Charles Drake were elected permanent members.

*Annual Medical Commencement of the University of the State of New-York.*

Agreeably to a resolution of the Honourable the Regents of the University of the State of New-York, the annual commencement, for the purpose of conferring the degree of *Doctor in Medicine* in the College of Physicians and Surgeons in this city, was held on the third of April, 1821. The exercises took place in the Hall of the University, in Barclay-street, and a numerous and respectable audience honoured the occasion with their presence. Conformably to the decision of the Regents of the University, the degree of Doctor in Medicine was conferred on the following

*thirty gentlemen, who had been students of the College, and had complied with the requisitions of the laws of the state, and of the University, regulating the practice of Physic and Surgery, and publicly defended their respective inaugural dissertations. Prayers were offered up by the Right Reverend Bishop Hobart; the other exercises were performed by the senior Professor of the College, Dr. Hosack.*

*Salmon A. Arnold, of Rhode Island. Gerard Bancker, of New-York. Hersey Baylies, of New-York. William A. L. Collins, of Georgia. James Demarest, of New-Jersey. Thomas I. Eppse, of Virginia. Ralph E. Elliot, A.B. of South Carolina. Edward H. Fisher, of South Carolina, Wilson Faulke, of Tennessee. Robert L. Green, A. B. of South Carolina. Robert Greenhow, of Virginia. Lana I. Hancock, of South Carolina. William Hume, of South Carolina. William Ley, A. B. of South Carolina. Thomas L. Lamar, of Georgia. Lueco Mitchill, of North Carolina. Elijah Mead, of Massachusetts. Alexander M. Montgomery, of New-Jersey. James M'Farlane, of South Carolina. John Neilson, Jun. A. B. of New-York. Richard Pennell, of New-York. J. Smyth Rogers, A. B. of New-York. Jacob Schmidt, of South Carolina. Jacob S. Swann, of Virginia. Samuel S. Treat, of New-York. Henry A. Tatum, of Virginia. John Allen Taylor, A. M. of New-Jersey. Abraham D. Wilson. A. B. of New-York. Robert C. Wood, of Rhode Island. Reuben C. Worthington, A. B. of South Carolina.*

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*A Card.*

TO THE EDITORS OF THE PHILADELPHIA MEDICAL RECORDER.

It is neither the intention, nor the task of the subscriber, to vindicate the Report of the Medical Society against the attack in paper XII of your last Number. It is a kind of polemic performance, which it would be difficult to follow

up, and oppose on the footing of the least degree of that liberality and dignity, which are indispensable in a scientific journal.

My name, however, having been singled out, for some purpose, as a member of the committee who drew up the Report, and as the author of a former publication,\* I think it incumbent upon me to remonstrate against a gross misrepresentation of my opinions respecting the causes of yellow fever, which I have uniformly defended during thirty years. No person has a right to pervert them, any more than to invade one's property or reputation. Indeed, I never apprehended such an attempt from professional gentlemen; and that it is a violation of the ordinary rules of justice, the following must manifestly prove.

Great pains have been taken to show, that I have attributed yellow fever to a cause, which is plainly rejected in the writings of Rush and Miller; and also, that in less than twelve months, I have *flatly contradicted* one absurdity by another. In order to come at these imputations, it is represented, that the committee who drew the Medical Report, attributed the yellow fever to the *operation of heat*; but that I had, one year before, already assigned the fatal distemper to the *operation of moisture*. If, in less than one year, I had thus changed my opinion from *moisture to heat*, it is, by the bye, very curious that I should have become, as the Recorder says, *a prominent member of the committee*; and that, in *defending their report in toto before the Medical Society*, no one should have invalidated my defence by my own inconsistency! But let us inquire further into this profound piece of criticism.

The committee having estimated upon the best authority, (page 8) that an immense pestiferous *steam* can be produced by a great number of people encompassed within a

\* A statement of the occurrences during a Malignant yellow fever in the City of New-York.—1819. &c.

small space of ground, have said, that "*a long continued intense heat, or calm and dry weather, when assisted by impure miasmatic exhalations, was one of the predisposing causes, &c.*" (page 12.) It was asserted by Rush, that yellow fever appears when heat acts upon moist animal and vegetable matter. Miller added, that for its production, *it is necessary that there should be a concurrence of heat, moisture, and a quantity of decaying animal and vegetable matter.* All which is in unison with the Medical Report on the subject of the operation of heat on the clustered and decayed habitations, many of which were crowded with hundreds of wretched tenants, incessantly dropping filth from their bodies, eating, drinking, and affording abundant moisture for the process of exhalation. It is therefore false, that the Committee, and with them Dr. P., have attributed yellow fever to *heat*, independently of moisture or impure exhalation.

But, the second term of the comparison, which is adduced to prove a *flat contradiction* of the Committee to what one of its members had said one year before, that is, yellow fever having been ascribed to moisture only, is truly astonishing.

There are no less than four pages in my Statement of the epidemic in New-York of 1819, wholly dedicated to the explanation of the laws and nature of miasmatic exhalations; of their varied operations and circumstances. This explanation commences at page 21, in the following words; "It may be said, that if such an operation of the causes we have pointed out," (heat and moisture on a great quantity of fermentable materials) &c. The first paragraph accounts for the *excessive heats devouring* or destroying the putrescible materials, which might engender pestilence, if they were not carried off by evaporation. It is there also related, that *plague itself is often checked by excessive heats.* The second and fifth paragraphs further illustrate the operation of less degrees of



heat, and moisture, which determine the formation of the different kinds of fever. It is not, therefore, my fault, if this part of the Statement has not been read by our learned antagonist. But according to this doctrine, it was consistent to have premised, in page 19, that *large cities are healthier with excessive and dry heats*; and that *with much rain and showers*, New-York had probably been in 1819, very sickly *on account of great quantities of putrescible materials in different parts of the city*. Now, what is the conclusion drawn by a disingenuous critic? Why forsooth, that Dr. Pascalis attributes the yellow fever to moisture!

After such a distortion of my professed opinion on those primordial laws of nature which must necessarily create malignant fevers; after such a misuse of detached words and disconnected sentences, from a series of arguments, by a writer who to say the least, either rejects them, or is not acquainted with their import. I leave it with the Editors of the Recorder to consider the propriety and justice of this concluding sentence, in page 321 of their last number; "a more flagrant specimen of contradiction cannot be met with in the records of inconsistency and absurdity."

The subscriber is aware that this fabrication is not the joint work of those, who did him the honour eighteen months ago, to associate his name with the names of those eminent persons who have contributed to improve the general opinions concerning the causes of the epidemic diseases of this country!\* I admit, also, that no medical character, not even a fellow labourer in the task of diffusing useful knowledge, should expect to escape in your pages the shaft of controversy, or merited censure; yet may I be allowed to ask, whether they should be equally opened to combined and studied misrepresentations from conflicting parties, without even common regard to man-

\* American Medical Recorder; October, 1819. p. 475.

ners and persons, and, as if their names and labours had really incurred the vituperation of *audacity, senselessness, absurdity* and *ignorance*? Were I to think so, I should not do justice to the feelings I have hitherto entertained, and to my perfect conviction of contrary rules and dispositions on your part. To the author of the paper complained of, the following memento is therefore recommended ;

" Sylvis deducti caveant, me iudice, Fauni,  
Ne velut innati triviis, ac penè forenses,  
Aut nimium teneris juvenentur versibus unquam,  
Aut immunda crepent, ignominiosaque dicta."

*Hor. de arte poetica.*

FELIX PASCALIS.

New-York, April, 1821.

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*Card.*

TO THE EDITORS OF THE MEDICAL REPOSITORY.

I feel as if I ought to ask pardon of the readers of the Medical Repository, for again obtruding myself upon their attention, with a subject, that more immediately concerns myself; but as Dr. Watts, the *reporter* of the cases published in the Medical and Surgical Register, of the supposed effects of drinking cold water, as alluded to in the two last numbers of the Repository, has thought proper to assert, that it is proved beyond doubt, that the cases in question, were not prescribed for, and treated under my sole, and immediate direction; I flatter myself that I shall be considered in the same degree excusable, for coming forward to repel this assertion, as I was justifiable in the first instance, in undeceiving the reader in regard to the very ingenious method adopted by the *reporter*, for securing to himself, a credit, to which I well knew, and still know, he was not entitled.

With as much propriety, might I, on entering upon my professional duties in the hospital, take up a case of chorea, for example, which had remained in the ward, and which had been treated by my predecessor, in a peculiar manner successfully, and whom I found in a state of convalescence; and sometime after, publish this case to the world, as a case of my own particular management.

I have asserted, and still maintain, that in the summer of 1818, I was the first, who in the New-York Hospital, prescribed, directed, and established the method of treatment, which, in the cases in question, proved so eminently successful. In contradiction to this, Dr. Watts has produced three statements, or if he pleases, three certificates, from Drs. Helme, Pendleton, and Campbell. With regard to the first, I would remark, that Dr. Helme does not pretend to say, that the cases referred to by him, were the first which had occurred, or which had been received into the Hospital that summer; and as to his *impression*, that it was Dr. Watts, who recommended the course of treatment to which they were subjected; any one who has read Dr. Watts's Report, can easily understand, how impressions favourable to one's purpose, can be made on the minds of others by the very simple process of suppressing a few material facts, and circumstances.

As Dr. Pendleton's statement, is derived wholly, from the misrepresentations of Dr. Campbell, and as I shall proceed presently, to prove the gross inaccuracy of Dr. Campbell's statement on the subject, all which Dr. Pendleton has communicated, is entitled to very little weight.

In order to introduce the proof of Dr. Campbell's incorrectness, I have thought proper to give a brief statement of facts, in confirmation of which, I shall subjoin the declarations of several respectable witnesses, two of them testifying to the same fact, (that of the mason who was brought from the coal vault, in the Hospital-yard) in con-

tradiction to him; and which furnish a degree of evidence that must be allowed to prevail.

In the latter part of the month of June, 1818, in the afternoon, a patient was received into the New-York Hospital. Being the attending physician, in consequence of the extreme illness of the man, Dr. Campbell, the resident, (as was his duty,) sent a messenger, requesting my immediate attendance. I repaired immediately to the Hospital, where I found the patient labouring under the symptoms of apoplexy; and on inquiry, was informed that he had imprudently taken a large draught of cold water, whilst very warm. Dr. Campbell, had exhibited a dose of laudanum before I arrived, which was immediately rejected from the stomach; his respiration was laborious, and stertorous; pulse at the wrist, small, but the heart, and vessels of the neck, beating violently. I immediately directed a vein to be opened in the arm, and applied my fingers to the wrist on the opposite side, and very soon found the pulse begin to rise, and become fuller. The blood was allowed to flow until the respiration was relieved; the arm was then bound up, stimuli with frictions, were applied to the surface of the body, and a warm bath directed to be prepared. The breathing again, very soon became laborious, and stertorous. The arm was again tied up, and the blood permitted to flow; spirits of hartshorn, was applied to the chest, and to the nape of the neck; the breathing was again relieved, and the blood stopped: he was then put into a tub of warm water, and several men were employed in rubbing the body and limbs, with woollen cloths and soap. All this time I remained with the patient, and directed the treatment; in a short time, and while in the bath, the breathing again became laborious and stertorous, the blood was again permitted to flow from the arm, until the breathing was relieved: he was then taken out from the bath, put to bed, and sinapisms were applied to the feet. Very soon the re-



spiration again became laborious, but not so much so as before, and he was a fourth time bled, and relieved; a blister was then directed to be applied to the nape of the neck, and I left him under the care of Dr. Campbell, and a faithful and intelligent nurse. At nine o'clock P. M. I again visited him, and found him tolerably comfortable, directed some light nourishment, (arrow root with a little wine) and left him for the night.

I found him much improved, and doing well; at my request the blood which was drawn from the arm of this man, was measured, and found to amount to eighty two ounces, this quantity was taken in the short space of two hours. The man recovered rapidly, without any unpleasant symptom occurring, during his convalescence.

This was the first patient who was received into the Hospital, during this extreme warm weather, labouring under those unpleasant, and alarming symptoms, and established the method of treatment which was pursued, with others; who were received in the Hospital afterwards: all of whom were placed under my immediate direction.

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*The Testimony of Mrs. Patterson, the Nurse.*

The above statement having been read over to me in the presence of Mr. Noah Wetmore, the superintendant of the Hospital, in which I have served as nurse, for nearly twenty years; and was the nurse in the ward into which the patient was brought. I do hereby declare that the circumstances above stated are correct and true; that I very well recollect the circumstance of the patient referred to; and of Doct. Neilson being sent for; of his coming immediately, attending to, and prescribing for said patient in the manner above mentioned; that I attended to all the directions of Doct. Neilson, in rela-

tion to the treatment and cure of said patient, and that no other physician, except Doct. Campbell the resident, was present on the occasion. And I particularly remember, that the patient in question was the first one of the kind that was received into the Hospital, in the summer of 1818, and that the treatment, as above described, was the first instance, to my knowledge, of that mode of practice in this hospital.

I further declare, that I perfectly well remember the case that afterwards occurred, of a workman employed in making a coal vault in the Hospital yard, who having drank too freely of cold water, while he was very warm, was seized with similar symptoms, so that it became necessary to have him brought into the Hospital. He was brought into my ward; I assisted in carrying him into the room; then called to Dr. Neilson, who was in the hospital at the time, and told him that here was another case, meaning a case similar to the one before mentioned. I remember that he ordered him to be bled, and some medicine to be given to him; that the man recovered very soon, and that he was attended by no other physician, than Dr. Neilson, and Dr. Campbell acting under his (Dr. Neilson's) directions.

Elizabeth Patterson.

In the presence of Noah Wetmore,  
Sup. of N. Y. Hospital. 20th *February*, 1821.

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*The Testimony of Mr. Wetmore.*

I distinctly recollect, that in the beginning of July, 1818, while we were constructing a vault for the reception of coal, one of our masons, while very warm, drank too freely of cold water, and was suddenly seized, with symptoms of apoplexy. Dr. Neilson being at the time in the hospital, (In consequence of the number and danger of the patients,

who were brought in during these few very warm days, Dr. Neilson attended at the house, several times in the course of each day) I called him and observed, here is another case Doctor; he had him immediately carried into the ward, and directed Dr. Campbell the resident, to bleed him freely, and to give him some medicine, the man recovered, and returned to his work in two days. It was my impression then, and it is my belief now, that Dr. Neilson, prescribed for, and directed the course of treatment for the first cases, which were received in June, and July, 1818, he being the attending physician.

Noah Wetmore, Sup.

N. Y. 20th February, 1821.

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*Mr. Samuel Wetmore's Statement.*

I have been clerk of the Hospital for five years past. It was my impression at the time, and it is my belief now, that Dr. Neilson, prescribed for, and directed, the treatment of those first cold water cases, which were received into the New-York Hospital in June and July, 1818.

N. Y. 20th Feb. 1821.

S. Wetmore, Clerk,

A remark or two now, as to the strength and weight of the evidence on both sides; and first, as to our own individual assertions. Dr. Watts, neither in his *report*, nor in his *card* has ventured positively to assert, that he, in the New-York Hospital, prescribed for, and directed the treatment of the first cases of the disease alluded to; though his *report* is ingeniously calculated to insinuate that belief; and in his *card*, he merely asserts that it is proved; these cases were not prescribed for, and treated under my sole and immediate direction, and that there is abundant evidence, that he treated *many* of the first cases. Now, as the material question between us, is, who treated the first case, that established the method of cure, it is not necessary to dispute whether he prescribed for any, or how

many of the cases, subsequent to the first, because from the number of cases which were received, several that succeeded the first, may without impropriety be described as *some*, or as Dr. Watts expresses it, *many of the first*; though the first case be actually excluded.

Now, all the evidence I have to contend with, is the statement of Dr. Campbell, which I think is completely invalidated, by the following fact, in addition to the evidence I have above set forth. He states the first case to have occurred on the 30th of June, but Dr. Watts himself in his report, states that the cold water cases occurred on the 29th and 30th of June and 1st of July, the case therefore of the 30th could not have been the first case, and as the patient whom I first attended, and prescribed for, had already rejected the laudanum administered to him by Dr. Campbell, (as mentioned in my statement of the case) it is pretty clear that Dr. C. had not yet had an opportunity of learning from Dr. Watts this mode of treatment, or he would hardly have disregarded in so short a time, the lesson of one, for whose professional credit it seems, he has been willing and ready, to say so much.

Dr. Campbell in order to prove the correctness of his recollection, as to his following Dr. Watt's directions, particularizes the instance of the mason, employed at the coal vault in the Hospital-Yard; but unfortunately for his selection, he has hit upon the very case that happens to be fully, and distinctly remembered, from its peculiar circumstances by two witnesses quite as competent as himself, and both of which, corroborate my statement.

I think therefore I am warranted in saying that Doct. Campbell's statement, in which is comprised the whole of Doct. Watts's evidence, is not only invalidated, but wholly disproved, and so far from having been incautious in my assumptions, as Doct. Watts is pleased to say, I have placed myself on ground too solid, and too well guarded to require that any thing which I have said, should be retracted.

JOHN NEILSON.



APRIL, 1820.

THERMOMETER.

THERMOMETER.																		
Places.	Lat.		Long.	Highest degree.			Lowest degree.			Mean temperature.			Hottest day.	Coldest day.				
	W.	E.		W.	VII.		II.		IX.	W.	VII.				II.		IX.	
					W.	E.	W.	E.			W.	E.			W.	E.		
Plattsburgh, N.Y.	44	42	73	25	72	66	56	25	28	20	44	40	51	10	43	10	Ths. 20.	Sun. 2.
St. Peters,	44	01	93	30	74	85	60	10	26	20	47	93	61	33	48	90	Mon. 24.	Sun. 2.
Sackett's Harbour,	43	55	75	57	60	74	70	22	32	30	43	63	51	33	49	73	Sat. 22.	Mon. 3.
Portsmouth, N. H.	43	05	70	45		64			24				42	00				
Boston,	42	22	71	04	43	75	56	27	43	34	40	26	53	80	43	65	Tues. 13.	Wed. 5.
Prairie du Chien,	42	43	91	40	74	88	77	12	36	29	49	20	65	73	54	65	Mon. 24.	Mon. 3.
Council Bluffs,	41	31	96	42	60	94	62	21	34	23	52	03	63	90	53	10	Mon. 10.	Sun. 2.
Pittsburgh Arsenal, Pa.	40	32	30	08	62	81	78	31	32	30	50	96	60	60	57	46	Fri. 21.	Sun. 2.
Frankford Arsenal, Pa.	39	40			63	89	68	21	38	20	47	40	62	10	48	46	Ths. 20.	Mon. 3.
Fort Washington, Md.	38	52	77	02	68	88	78	29	33	32	52	16	62	60	57	40	Fri. 21.	Mon. 3.
Fort Johnston, N. C.	33	54	78	10	70	82	70	48	52	32	64	50	69	93	65	83	Mon. 24.	Mon. 3.
Amelia Island,	30	45	81	57	72	85	77	45	55	51	71	60	75	03	68	03	Wed. 26.	Tues. 4.
Fort Scott, Geo.	30	43	84	38	72	89	74	44	61	51	60	23	77	26	68	33	Wed. 26.	Tues. 4.
Cant. Montpelier, Al.	31	12	87	48	68	84	69	39	60	47	61	36	75	56	67	36	Wed. 26.	Tues. 4.
Baton Rouge.	30	36	91	13	72	86	84	42	54	52	64	83	74	66	71	33	Tues. 25	Mon. 3.
New-Orleans,	35	02	90	10	75	91	78		58	63	73	71	76	64	74	17	Wed. 26.	Mon. 3.
Fort Gadsden, E. F.	29	58	84	50	66	86	74	42	62	53	62	93	77	77	66	66	Tues. 25.	Tues. 4.
Camp Ripley,	31	18	93	50	72	87	77	55	78	68	68	63	83	18	74	81	Mon. 24.	Ths. 27.



MAY, 1820.

## THERMOMETER.

THERMOMETER.															
Places.	Highest degree.			Lowest degree.			Mean temperature.							Hottest day.	Coldest day.
	VII.		IX.	VII.		II.	IX.		VII.	II.		IX.			
	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.			
Plattsburgh, N. Y.	56	76	63	44	45	44	54	90	61	54	54	09	Fri. 5.	Fri. 26.	
St. Peters,	62	82	68	36	46	36	55	65	70	57	55	70	Sat. 20.	Sun. 7.	
Sackett's Harbour,	58	70	63	22	32	30	48	41	54	67	52	80	Ths. 11.	Wed. 3.	
Portsmouth, N. H.		72			45				54	70					
Boston,	41	78	59	52	42	41	51	13	64	74	52	29	Wed. 10.	Fri. 26.	
Prairie du Chien,	68	90	67	39	52	50	53	93	71	60	57	13	Tues. 23.	Sun. 7.	
Council Bluffs,	69	90	71	52	55	50	58	38	71	38	58	41	Tues. 23.	Wed. 10.	
Pittsburg Arsenal, Pa.	56	82	75	40	56	52	52	35	61	80	59	29	Tues. 23.	Tues. 16.	
Frankford Arsenal, Pa.	66	82	68	45	62	51	56	19	67	45	58	09	Wed. 24.	Sat. 27.	
Fort Washington, Md.	70	84	72	51	56	54	58	83	67	80	62	32	Wed. 24.	Ths. 18.	
Fort Johnston, N. C.	77	82	72	60	67	61	65	96	71	54	68	61	Fri. 26.	Sat. 27.	
Amelia Island,	74	86	76	61	76	69	68	70	80	12	72	38	Sat. 13.	Tues. 2.	
Fort Scott, Geo.	65	88	79	56	82	69	63	61	81	83	71	32	Ths. 18.	Tues. 2.	
Cant. Montpelier, Al.	69	90	76	59	73	68	67	09	80	16	71	74	Wed. 24.	Mon. 1.	
Baton Rouge,	72	86	90	58	76	78	61	61	80	90	81	06	13 & 20th	Tues. 2.	
New-Orleans,	81	87	81	72	76	74	77	18	81	58	79	20	Sat. 13.	Mon. 1.	
Fort Gadsden, E. F.	60	84	91	60	84	91	67	96	81	19	71	67	Ths. 4.	Ths. 4.	
Camp Ripley.	72	88	84	54	75	72	71	29	81	35	75	51	Ths. 25.	Tues. 2.	

MAY, 1820.

Places.	WINDS.										WEATHER.			
	N. W.					E. S.					Prevailing.			
	N.		W.		days	E.		S.		days	Fair. Clo. Rain Snow			
	days	days	days	days		days	days	days	days		days	days	days	days
Plattsburgh, N. Y.	5	2	6	1	3	12	3	6	1	S.	24	7	2	Fair.
St. Peters,	1	4	6		5	6		2	6	SE.	21	2	3	Fair†
Sackett's Harbour,		5	6			2		2	7	W.	12	9	10	Fair.
Portsmouth, N. H.	4	3	9	7	2	3		3	3	E.	15	16		Clo.
Boston,		13	1	2	5	5		1	2	NE.	13	6	12	Fair.
Prairie du Chien,	3	2	5	8	12				1	NW.	19	5	7	Fair.
Council Bluffs,	2	1		2					7	SE.	19	3	9	Fair.
Pittsburgh Arsenal, Pa.	2	10	7	2		2		2	17	W.	17	6	8	Fair.
Frankfort Arsenal Pa.	4	6	4	2	4	1		1	9	NW.	10	12	9	Clo.
Fort Washington, Md.	1	4	2	2	4	4		3	1	SW.	14	8	9	Fair.
Fort Johnston, N. C.	1	2			23	3		5	5	SW.	25	4	2	Fair.
Amelia Island,				8						SE.	22		9	Fair.
Fort Scott, Geo.				2		3		20	20	W.	19	12		Fair.
Cant. Montpalier, Al.		9	2	2	7	2		7	2	NW.	16	5	10	Fair.
Baton Rouge.		4	6		20			1		SE.	15	9	7	Fair†
New-Orleans											17	4	10	Fair.
Fort Gadsden, E. F.		7		2	6	3		6	7	NW.	9	13	9	Clo.
Camp Ripley.		2	6		7	3		10	3	SW.	14	5	11	Fair.



JUNE, 1820.

Places.	THERMOMETER.												Hottest day.	Coldest day.						
	Highest degree.						Lowest degree.								Mean temperature.					
	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.								
Plattsburgh, N. Y.	88	90	75	50	66	52	67	46	74	10	65	14	Thur. 22	Fri. 9.						
St. Peters.	75	82	78	52	82	54	65	86	80	10	66	63	20 & 28th	Mon. 5.						
Sackett's Harbour,	70	84	84	50	61	60	60	90	69	03	66	06	Tues. 20	Wed. 7.						
Portsmouth, N. H.		94			52			67	06											
Boston,	78	98	82	50	74		58	10	77	31	66	22	Fri. 30	Thur. 1.						
Prairie du Chien,	78	99	89	50	76	59	68	55	84	43	71	69	Sun. 18	Tues. 6.						
Council Bluffs,	77	99	74	56	76	55	70	60	82	85	68	90	Thur. 29	Thur. 1.						
Pittsburgh Arsenal, Pa.	74	90	80	54	67	63	64	23	76	60	71	90	Fri. 30	Tues. 6.						
Frankfort Arsenal, Pa.	79	95	85	56	68	59	66	93	81	16	69	63	Fri. 30	Thur. 1.						
Fort Washington, Md.	73	92	80	56	67	62	69	53	79	30	74	20	Sat. 24	Thur. 1.						
Fort Johnston, N. C.	78	84	82	64	72	64	73	50	79	13	73	50	Sat. 24	Sun. 4.						
Amelia Island,	79	87	83	67	77	69	75	40	83	86	76	00	Thur. 15	Fri. 2.						
Fort Scott, Geo.	75	91	80	60	67	62	73	20	84	93	76	96	Thur. 29	Fri. 2.						
Cant. Montpelier, Al.	66	94	84	58	66	64	73	96	85	16	77	80	Sun. 11.	Fri. 2.						
Baton Rouge,	77	93	94	60	76	78	73	90	86	10	84	26	Sun. 18	Fri. 2.						
New-Orleans,	84	91	85	72	75	74	80	50	84	93	82	46	Sun. 18	Fri. 2.						
Fort Gadsden, E. P.	77	89	72	57	75	67	71	56	82	40	73	66	Sat. 17	Sat. 3.						
Camp Ripley.	78	92	80	57	74	69	70	10	85	50	79	93	Mon. 19	Fri. 2.						

## JUNE, 1820.

Places.	WINDS.										WEATHER.				Prevailing q.		
	N.		NW.		NE.		E.		SE.		S.		SW.			W.	
	days	days	days	days	days	days	days	days	days	days	days	days	days	days		days	
	days	days	days	days	days	days	days	days	days	days	days	days	days	days		days	
Plattsburgh, N.Y.	8																Fair
St. Peters,	1	3			1				2	10	5		3			2	SE.
Sacket's Harbour,	1				2				6	3	3		14			4	SW.
Portsmouth, N. H.	1	10			1		2		3	3	2		2			9	NW.
Boston,	9	1			1		3		1	1	2		9			4	N & SW
Prairie du chien,	2	6							12	1	1		3			1	SE.
Council Bluffs,	2	2			3		4		10	4			5				SE.
Pittsburg Arsenal, Pa.	6	1					8		3				9			3	SW.
Frankford Arsenal, Pa.		10			8		1		3				6			2	NW.
Fort Washington, Md.	9	3			6					1			6			5	N.
Fort Johnston, N. C,					9		5		2	14			2			1	S.
Amelia Island,					1		1		25								SE.
Fort Scott, Geo.							6			9			15				W.
Cant. Montpelier, Al.		11			1				3		2		9			3	NW.
Baton Rouge,	1	3			2				14	4			6				SE.
New-Orleans,																	
Fort Gadsden, E. F.		2			1		2		3		10		8			4	S.
Camp Ripley,		1			5		2		6		2		11			3	SW.

*Remarks by Joseph Lovell, Surgeon General U. S. A.*

The mean temperature for April is 61.31; for May 66.07; for June 74.61; and for the quarter 67.31. In April, the prevailing winds were S.E., S. and S.W.; in May, S.E., N.W. and S.W.; in June, S.E., S. and S.W.; and for the quarter, S.E. S.W. and S., with but little variation. Out of 17 places of observation, in April, the prevailing weather was fair on the whole; and out of 18 places in May and June, the prevailing weather was fair at 15 in the former, and 16 in the latter month. In April, the proportion was about  $18\frac{1}{2}$  fair, 7 cloudy, 4 rain and  $\frac{1}{2}$  snow; in May,  $16\frac{3}{4}$  fair, 7 cloudy, 7 rain; in June,  $17\frac{1}{2}$  fair, 6 cloudy, 6 rain; and for the quarter  $52\frac{3}{4}$  fair, 20 cloudy,  $17\frac{1}{4}$  rain, and a fraction snow.

It appears that the temperature at the eastern posts increases gradually and regularly through the quarter; and generally in direct proportion to their latitudes: but we find the increase greater and much more rapid at the western posts, whether compared with those north or south of them, or with those in the same latitude. Thus, the mean temperature at St. Peter's at 2 P. M. is twelve degrees above that at Sackett's Harbour, and eight degrees above that at Plattsburgh; the first being but 46 minutes further south, and the latter but 41 minutes further north. In January the mean temperature at the same time was  $19^{\circ}$  below that at Sackett's Harbour, in February, but  $5^{\circ}$  in March the same, in April  $10^{\circ}$ , in May  $13^{\circ}$ , and in June  $11^{\circ}$  higher. Again: at Prairie du Chien, which is but a few miles north, and  $20^{\circ}$  west of Boston, the mean temperature at 2 P. M. is  $3\frac{1}{2}^{\circ}$  higher—in April it is  $3^{\circ}$  above that at Fort Washington on the Potomac, in May equal to that at Fort Johnston, North Carolina, and in June equal to that at New-Orleans, which is  $12^{\circ} 41'$  further south: whereas, in Boston, it was in April  $9^{\circ}$  below that at Fort Washington, in May  $7^{\circ}$  below that at Fort Johnston, N. Carolina, and in June,  $7\frac{1}{2}^{\circ}$  below that at New-Orleans.

The proportion of fair weather is very nearly the same as during the first quarter; and it appears to be pretty equally distributed through the country. The greatest number of fair days (70) was at Plattsburgh, in the State of New-York, and Fernandina, on Amelia Island; the smallest number was 34, at the cantonment on the Sabine river, about 400 miles N.W. from New-Orleans; and 33 at Fort Gadsden, in Florida, near the mouth of the Appalachicola river.

The snow, in April, extended from Sackett's Harbour in latitude  $43^{\circ} 55'$  to Fort Washington, in latitude  $38^{\circ} 52'$  and from Boston, in longitude  $71^{\circ} 04'$  to Council Bluff's in longitude  $96^{\circ} 42'$ . The extent of country shows the agency of general causes, but at the time at which it fell at the several places shows these to have been in some measure modified by local circumstances; for it fell on the 1st at Council Bluffs, and the 6th at Prairie du Chiens; on the 2d at Pittsburgh, Frankford, and Fort Washington; on the 5th and 7th at Sackett's Harbour, and on the 7th at Boston. The two days of snow at St. Peter's, in May, were on the 6th and 7th.

During the first quarter, it appeared the prevailing winds were from the N.W. with a greater proportion from the south, as the season advanced. During this quarter, they were pretty steadily from the S.E., S. and S.W. It will also be observed that northerly winds have occurred much more frequently at the eastern than at the western posts.